PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCII.

PART II.

LONDON,

PRINTED BY W. BULMER AND CO. CLEVELAND-ROW, ST. JAMES'S;
AND SOLD BY G. AND W. NICOL, PALL-MALL, BOOKSELLERS TO HIS MAJESTY,
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PHILOSOPHICAL
TRANSACTIONS.

VIII. *Observations on the two lately discovered celestial Bodies.* By William Herschel, *LL. D. F. R. S.*

Read May 6, 1802.

In my early account of the moving star discovered by Mr. Piazzi, I have already shewn that it is of a remarkably small size, deviating much from that of all the primary planets.*

It was not my intention to rest satisfied with an estimation of the diameter of this curious object, obtained by comparing it with the Georgian planet, and, having now been very successful in the application of the lucid disk micrometer, I shall relate the result of my investigations.

But the very interesting discovery of Dr. Olbers having introduced another moving star to our knowledge, I have extended my researches to the magnitude, and physical construction, of that also. Its very particular nature, which, from the observations I shall relate, appears to be rather cometary

* By comparing its apparent disk with that of the Georgian planet, it was estimated, that the real diameter of this new star could not amount to $\frac{1}{3}$ths of that of our moon.

than planetary, will possibly throw also considerable light upon the circumstances belonging to the other celestial body; and, by that means, enable us to form some judgment of the nature of both the two last-discovered phenomena.

As the measures I have taken will oblige me to give a result which must appear extraordinary, it will be highly necessary to be particular in the circumstances of these measures, and to mention the condition and powers of the telescopes that were used to obtain them.

Magnitude of the new Stars.

April 1, 1802. Having placed a lucid disk at a considerable distance from the eye, but so that I might view it with perfect distinctness, I threw the image of Mr. PIAZZI's star, seen in a 7-feet reflector, very near it, in order to have the projected picture of the star and the lucid disk side by side, that I might ascertain their comparative magnitudes. I soon perceived that the length of my garden would not allow me to remove the disk-micrometer, which must be placed at right angles to the telescope, far enough to make it appear no larger than the star; and, not having disks of a less diameter prepared, I placed the smallest I had, as far from me as the situation of the star would allow. Then, bringing its image again by the side of the disk, and viewing, at the same time, with one eye the magnified star, while the other eye saw the lucid disk, I perceived that Ceres, which is the name the discoverer has given to the star, was hardly more than one third of the diameter of the disk, and certainly less than one half of it.

This being repeated, and always appearing the same, we

shall not under-rate the size of the star, by admitting its diameter to have been 45 hundredths of the lucid disk.

The power of the telescope, very precisely ascertained, by terrestrial geometrical measures properly reduced to the focus of the mirror on the stars, was 370,42. The distance of the lucid disk from the eye, was 2131 inches; and its diameter 3,4 inches. Hence we compute, that the disk was seen under an angle of 5′ 29″,09; and Ceres, when magnified 370 times, appearing, as we have shewn, 45 hundredths of that magnitude, its real diameter could not exceed 0″,40. Had this diameter amounted to as much as was formerly estimated, the power of 370 would have made it appear of 6′ 10″, which is more than the whole lucid disk.

This extraordinary result, raised in me a suspicion, that the power 370 of a 7-feet telescope, and its aperture of 6,3 inches, might not be sufficient to shew the planet's feeble light properly. I therefore adapted my 10-feet instrument to observations with lucid disks; which require a different arrangement of the head of the telescope and finder: I also made some small transparencies, to represent the object I intended to measure.

April 21. The night being pretty clear, though perhaps not quite so proper for delicate vision as I could have wished, I directed my 10-feet reflector, with a magnifying power of 516,54, also ascertained by geometrical terrestrial measures reduced to the focus of the instrument on celestial objects, to Mr. PIAZZI's star, and compared it with a lucid disk, placed at 1486 inches from the eye, and of 1,4 inch in diameter. I varied the distance of the lucid disk many times; and fixed at last on the above-mentioned one, as the best I could find. There was, however, a haziness about the star, which resembled a faint

coma; and this, it may be supposed, must render the measure less satisfactory than it would otherwise have been.

From these data we compute, that the disk appeared to the natural eye under an angle of 3′ 14″,83; while Ceres, when magnified 516½ times, was seen by the other eye of an equal magnitude; and that consequently its real diameter, by measurement, was only 0″,38.

April 22. 11ʰ 38′, sidereal time. I used now a more perfect small mirror; the former one having been injured by long continued solar observations. This gave me the apparent diameters of the stars uncommonly well defined; to which, perhaps, the very favourable and undisturbed clearness of the atmosphere might contribute considerably.

With a magnifying power of 881,51, properly ascertained, like those which have been mentioned before, I viewed Dr. OLBERS's star, and compared it with a lucid disk of 1,4 inch in diameter, placed at 1514 inches from the eye, measured, like the rest of the distances, with long deal rods. The star appeared to me so ill defined, that, ascribing it to the eye-glass, I thought it not adviseable to compare the object, as it then appeared, with a well defined lucid disk. Exchanging the glass for that which gives the telescope a magnifying power of 516½, I found Pallas, as the discoverer wishes to have it called, better defined; and saw, when brought together, that it was considerably less in diameter than the lucid disk.

In order to produce an equality, I removed the disk to 1949 inches; and still found Pallas considerably less than the disk.

Before I changed the distance again, I wished to ascertain whether Ceres or Pallas would appear under the largest angle, especially as the air was now more pure than last night. On

comparing the diameter of Ceres with that of the lucid disk, I found it certainly less than the disk. By proper attention, and continued examination, for at least an hour, I judged it to be nearly $\frac{1}{4}$ of the lucid disk.

Then, if we calculate as before, it appears by this observation, in which there is great reason to place confidence, that the angle under which this star appeared, was only 0″.22. For, a lucid disk of 1,4 inch diameter, at the distance of 1942 inches, would be seen under an angle of 2′ 28″,7; three quarters of which are 1′ 51″,52. This quantity, divided by the power 516,54, gives 0″,2159, or, as we have given it abridged, 0″,22.

13^h 7′. I removed the micrometer to the greatest convenient distance, namely, 2136 inches, and compared Dr. OLBERS's star, which, on account of its great altitude, I saw now in high perfection, with the lucid disk. It was, even at this distance, less than the diameter of the disk, in the proportion of 2 to 3.

When, by long continued attention, the appearance of Pallas was reduced to its smallest size, I judged it to bear no greater proportion to the diameter of the lucid disk of the micrometer, than as 1 to 2.

In consequence of these measures, it appears that the diameter of Pallas, according to the first of them, is 0″,17; and, according to the last, where the greatest possible distinctness was obtained, only 0″,13.

If it should appear almost incredible that these curious objects could give so small an image, had they been so much magnified as has been reported, I can say, that curiosity led me to throw the picture of Jupiter, given by the same telescope and magnifying power, on a wall at the distance of 1318 inches, of which it covered a space that measured 12 feet 11 inches. I do not

mention this as a measure of Jupiter, for the wall was not perfectly at right angles to the telescope, on which account the projected image would be a little larger than it should have been, nor was I very attentive to other necessary minute circumstances, which would be required for an accurate measure; but we see at once, from the size of this picture, that the power of the telescope exerted itself to the full of what has been stated.

As we generally can judge best of comparative magnitudes, when the measures are, as it were, brought home to us; it will not be amiss to reduce them to miles. This, however, cannot be done with great precision, till we are more perfectly acquainted with the elements of the orbits of these stars. But, for our present purpose, it will be sufficiently accurate, if we admit their mean distances from the sun, as the most recent information at present states them; for Ceres 2,6024; and for Pallas 2,8. The geocentric longitudes and north latitudes, at the time of observation, were, for Ceres, about ♍ 20° 4′, 15° 20′; and for Pallas, ♍ 23° 40′, 17° 30′. With these data, I have calculated the distances of the stars from the earth at the time of observation, partly by the usual method, and, where the elements were wanting, by a graphical process, which is sufficiently accurate for our purpose. My computed distances were 1,634 for Ceres, and 1,8333 for Pallas; and, by them we find, that the diameter of Ceres, at the mean distance of the earth from the sun, would subtend an angle of 0″,35127; and that, consequently, its real diameter is 161,6 miles.

It also follows, that Pallas would be seen, at the same distance from the sun, under an angle of 0″,3199; and that its real diameter, if the largest measure be taken, is 147 miles; but, if we take the most distinct observation, which gives the

smallest measure, the angle under which it would be seen from the sun, will be only 0″,2399; and its diameter, no more than 110$\frac{1}{3}$ miles.

Of Satellites.

After what has just now been shewn, with regard to the size of these new stars, there can be no great reason to expect that they should have any satellites. The little quantity of matter they contain, would hardly be adequate to the retention of a secondary body; but, as I have made many observations with a view to ascertain this point, it will not be amiss to relate them.

Feb. 25. 20-feet reflector. There is no small star near Ceres, that could be supposed to be a satellite.

Feb. 28. There is no small star within 3 or 4 minutes of Ceres, that might be taken for a satellite.

March 4. 9^h 45′, sidereal time. A very small star, south-preceding Ceres, may be a satellite. See Plate V. Fig. 1. where C is Ceres, S the supposed satellite, *a b c d e f*, are delineation stars, *c* and *d* are very small. S makes nearly a right angle with them; *e* is larger than either *c* or *d*. There is an extremely faint star *f*, between *e* and *d*.

14^h 16′. Ceres has left the supposed satellite behind.

March 5. There are two very small stars, which may be satellites; see Fig. 2. where they are marked, 1st S, 2d S. The rest, as before, are delineation stars.

March 6. The two supposed satellites of last night remain in their situation, Ceres having left them far behind.

10^h 16′. There is a very small star, like a satellite, about 75° south-following Ceres. See Fig. 3. It is in a line from C to *b* of last night.

$11^h\ 20'$. Ceres has advanced in its orbit; but has left the supposed satellite behind.

March 30. $9^h\ 35'$. A supposed 1st satellite is directly following Ceres: it is extremely faint. A 2d supposed satellite is north-following. See Fig. 4. The supposed satellites are so small, that, with a 20-feet telescope, they require a power of 300 to be seen; and the planet should be hidden behind a thick wire, placed a little out of the middle of the field of view, which must be left open to look for the supposed satellites.

$12^h\ 17'$. Ceres has changed its place, and left both the supposed satellites behind.

March 31. $9^h\ 20'$. There is a very small star, on the north-preceding side of Ceres, which may be a satellite.

$11^h\ 50'$. Ceres has moved forwards in its path; but the supposed satellite remains in its former situation. The nearest star is 20″ of time from Ceres; so that, within a circle of 40″ of time, there certainly is no satellite that can be seen with the space-penetrating power of this instrument.

It is evident, that when the motion of a celestial body is so considerable, we need never be long in doubt whether a small star be a satellite belonging to it, since a few hours must decide it.

May 1. $12^h\ 51'$. I viewed Pallas with the 20-feet reflector, power 300; there was no star within 3′, that could be taken for a satellite.

Of the Colour of the new Stars.

Feb. 13. The colour of Ceres is ruddy, but not very deep.

April 21. Ceres is much more ruddy than Pallas.

April 22. Pallas is of a dusky whitish colour.

Of the Appearances of the new Stars, with regard to a Disk.

Feb. 7. Ceres, with a magnifying power of 516½, shews an ill defined planetary disk, hardly to be distinguished from the surrounding haziness.

Feb. 13. Ceres has a visible disk.

April 22. In viewing Pallas, I cannot, with the utmost attention, and under the most favourable present circumstances, perceive any sharp termination which might denote a disk; it is rather what I would call a nucleus.

April 28. In the finder, Pallas is less than Ceres. It is also rather less than when I first saw it.

Of the Appearances of the new Stars, with regard to an Atmosphere, or Coma.

April 21. I viewed Ceres for nearly an hour together. There was a haziness about it, resembling a faint coma, which was, however, easily to be distinguished from the body.

April 22. I see the disk of Ceres better defined, and smaller, than I did last night. There does not seem to be any coma; and I am inclined to ascribe the appearance of last night to a deception, as I now and then, with long attention, saw it without; at which times, it was always best defined, and smallest.

April 28. Ceres is surrounded with a strong haziness. Power 550.

With 516½, which is a better glass, the breadth of the coma beyond the disk may amount to the extent of a diameter of the disk, which is not very sharply defined. Were the whole coma and star taken together, they would be at least three times as

large as my measure of the star. The coma is very dense near the nucleus; but loses itself pretty abruptly on the outside, though a gradual diminution is still very perceptible.

April 30. Ceres has a visible, but very small coma about it. This cannot be seen with low powers; as the whole of it together is not large enough, unless much magnified, to make up a visible quantity.

May 1. The diameter of the coma of Ceres, is about 5 times as large as the disk, or extends nearly 2 diameters beyond it.

13^h 19′. 20-feet reflector; power 477. The disk of Ceres is much better defined than that of Pallas. The coma about it is considerable, but not quite so extended as that of Pallas.

May 2. 13^h 20′. Ceres is better defined than I have generally seen it. Its disk is strongly marked; and, when I see it best, the haziness about it hardly exceeds that of the stars of an equal size.

Memorandum. This may be owing to a particular disposition of the atmosphere, which shews all the stars without twinkling, but not quite so bright as they appear at other times. Jupiter likewise has an extremely faint scattered light about it, which extends to nearly 4 or 5 degrees in diameter.

April 22. Pallas, with a power of $88\frac{1}{2}$, appears to be very ill defined. The glass is not in fault; for, in the day time, I can read with it the smallest letters on a message card, fixed up at a great distance.

13^h 17′. The appearance of Pallas is cometary; the disk, if it has any, being ill defined. When I see it to the best advantage, it appears like a much compressed, extremely small, but ill defined, planetary nebula.

April 28. Pallas is very ill defined: no determined disk can

be seen. The coma about it, or rather the coma itself, for no star appears within it, would certainly measure, at first sight, 4 or 5 times as much as it will do after it has been properly kept in view, in order to distinguish between the haziness which surrounds it, and that part which may be called the body.

May 1. Pallas has a very ill defined appearance; but the whole coma is compressed into a very small compass.

$13^h\ 5'$. 20-feet reflector; power 477. I see Pallas well, and perceive a very small disk, with a coma of some extent about it, the whole diameter of which may amount to 6 or 7 times that of the disk alone.

May 2. $13^h\ 0'$. 10-feet reflector. A star of exactly the same size, in the finder, with Pallas, viewed with $516\frac{1}{2}$, has a different appearance. In the centre of it is a round lucid point, which is not visible in Pallas. The evening is uncommonly calm and beautiful. I see Pallas better defined than I have seen it before. The coma is contracted into a very narrow compass; so that perhaps it is little more than the common aberration of light of every small star. See the memorandum to the observation of Ceres, May 2.

On the Nature of the new Stars.

From the account which we have now before us, a very important question will arise, which is, What are these new stars, are they planets, or are they comets? And, before we can enter into a proper examination of the subject, it will be necessary to lay down some definition of the meaning we have hitherto affixed to the term planet. This cannot be difficult, since we have seven

patterns to adjust our definition by. I should, for instance, say of planets,

1. They are celestial bodies, of a certain very considerable size.

2. They move in not very excentric ellipses round the sun.

3. The planes of their orbits do not deviate many degrees from the plane of the earth's orbit.

4. Their motion is direct.

5. They may have satellites, or rings.

6. They have an atmosphere of considerable extent, which however bears hardly any sensible proportion to their diameters.

7. Their orbits are at certain considerable distances from each other.

Now, if we may judge of these new stars by our first criterion, which is their size, we certainly cannot class them in the list of planets: for, to conclude from the measures I have taken, Mercury, which is the smallest, if divided, would make up more than 135 thousand such bodies as that of Pallas, in bulk.

In the second article, their motion, they agree perhaps sufficiently well.

The third, which relates to the situation of their orbits, seems again to point out a considerable difference. The geocentric latitude of Pallas, at present, is not less than between 17 and 18 degrees; and that of Ceres between 15 and 16; whereas, that of the planets does not amount to one half of that quantity. If bodies of this kind were to be admitted into the order of planets, we should be obliged to give up the zodiac; for, by extending it to them, should a few more of these stars be discovered, still farther and farther deviating from the path of the earth, which

is not unlikely, we might soon be obliged to convert the whole firmament into zodiac; that is to say, we should have none left.

In the fourth article, which points out the direction of the motion, these stars agree with the planets.

With regard to the fifth, concerning satellites, it may not be easy to prove a negative; though even that, as far as it can be done, has been shewn. But the retention of a satellite in its orbit, it is well known, requires a proper mass of matter in the central body, which it is evident these stars do not contain.

The sixth article seems to exclude these stars from the condition of planets. The small comas which they shew, give them so far the resemblance of comets, that in this respect we should be rather inclined to rank them in that order, did other circumstances permit us to assent to this idea.

In the seventh article, they are again unlike planets; for it appears, that their orbits are too near each other to agree with the general harmony that takes place among the rest; perhaps one of them might be brought in, to fill up a seeming vacancy between Mars and Jupiter. There is a certain regularity in the arrangement of planetary orbits, which has been pointed out by a very intelligent astronomer, so long ago as the year 1772; but this, by the admission of the two new stars into the order of planets, would be completely overturned; whereas, if they are of a different species, it may still remain established.

As we have now sufficiently shewn that our new stars cannot be called planets, we proceed to compare them also with the other proposed species of celestial bodies, namely, comets. The criteria by which we have hitherto distinguished these from planets, may be enumerated as follows.

1. They are celestial bodies, generally of a very small size, though how far this may be limited, is yet unknown.

2. They move in very excentric ellipses, or apparently parabolic arches, round the sun.

3. The planes of their motion admit of the greatest variety in their situation.

4. The direction of their motion also is totally undetermined.

5. They have atmospheres of very great extent, which shew themselves in various forms of tails, coma, haziness, &c.

On casting our eye over these distinguishing marks, it appears, that in the first point, relating to size, our new stars agree sufficiently well; for the magnitude of comets is not only small, but very unlimited. Mr. PIGOTT's comet, for instance, of the year 1781, seemed to have some kind of nucleus; though its magnitude was so ill defined, that I probably over-rated it much, when, November 22, I guessed it might amount to 3 or 4″ in diameter. But, even this, considering its nearness to the earth, proves it to have been very small.

That of the year 1783, also discovered by Mr. PIGOTT, I saw to more advantage, in the meridian, with a 20-feet reflector. It had a small nucleus, which, November 29, was coarsely estimated to be of perhaps 3″ diameter. In all my other pretty numerous observations of comets, it is expressly remarked, that they had none that could be seen. Besides, what I have called a nucleus, would still be far from what I now should have measured as a disk; to constitute which, a more determined outline is required.

In the second article, their motions differ much from that of comets; for, so far as we have at present an account of the

orbits of these new stars, they move in ellipses which are not very excentric.

Nor are the situations of the planes of their orbits so much unlike those of the planets, that we should think it necessary to bring them under the third article of comets, which leaves them quite unlimited.

In the fourth article, relating to the direction of their motion, these stars agree with planets, rather than with comets.

The fifth article, which refers to the atmosphere of comets, seems to point out these stars as belonging to that class; it will, however, on a more particular examination, appear that the difference is far too considerable to allow us to call them comets.

The following account of the size of the comas of the smallest comets I have observed, will shew that they are beyond comparison larger than those of our new stars.

Nov. 22, 1781. Mr. PIGOTT's comet had a coma of 5 or 6′ in diameter.

Nov. 29, 1783. Another of Mr. PIGOTT's comets had a coma of 8′ in diameter.

Dec. 22, 1788. My sister's comet had a coma of 5 or 6′ in diameter.

Jan. 9, 1790. Another of her comets was surrounded by haziness of 5 or 6′ in diameter.

Jan. 18, 1790. Mr. MECHAIN's comet had a coma of 5 or 6′ in diameter.

Nov. 7, 1795. My sister's comet had a coma of 5 or 6′ in diameter.

Sept. 8, 1799. Mr. STEPHEN LEE's comet had a coma of not less than 10′ in diameter, and also a small tail of 15′ in length.

From these observations, which give us the dimensions of the comas of the smallest comets that have been observed with good instruments, we conclude, that the comas of these new stars, which at most amount only to a few times the diameter of the bodies to which they belong, bear no resemblance to the comas of comets, which, even when smallest, exceed theirs above a hundred times. Not to mention the extensive atmospheres, and astonishing length of the tails, of some comets that have been observed, to which these new stars have nothing in the least similar.

Since, therefore, neither the appellation of planets, nor that of comets, can with any propriety of language be given to these two stars, we ought to distinguish them by a new name, denoting a species of celestial bodies hitherto unknown to us, but which the interesting discoveries of Mr. PIAZZI and Dr. OLBERS have brought to light.

With this intention, therefore, I have endeavoured to find out a leading feature in the character of these new stars; and, as planets are distinguished from the fixed stars by their visible change of situation in the zodiac, and comets by their remarkable comas, so the quality in which these objects differ considerably from the two former species, is that they resemble small stars so much as hardly to be distinguished from them, even by very good telescopes. It is owing to this very circumstance, that they have been so long concealed from our view. From this, their asteroidical appearance, if I may use that expression, therefore, I shall take my name, and call them *Asteroids*; reserving to myself, however, the liberty of changing that name, if another, more expressive of their nature, should occur. These bodies will hold a middle rank, between the two species that

were known before; so that planets, asteroids, and comets, will in future comprehend all the primary celestial bodies that either remain with, or only occasionally visit, our solar system.

I shall now give a definition of our new astronomical term, which ought to be considerably extensive, that it may not only take in the asteroid Ceres, as well as the asteroid Pallas, but that any other asteroid which may hereafter be discovered, let its motion or situation be whatever it may, shall also be fully delineated by it. This will stand as follows.

Asteroids are celestial bodies, which move in orbits either of little or of considerable excentricity round the sun, the plane of which may be inclined to the ecliptic in any angle whatsoever. Their motion may be direct, or retrograde; and they may or may not have considerable atmospheres, very small comas, disks, or nuclei.

As I have given a definition which is sufficiently extensive to take in future discoveries, it may be proper to state the reasons we have for expecting that additional asteroids may probably be soon found out. From the appearance of Ceres and Pallas it is evident, that the discovery of asteroids requires a particular method of examining the heavens, which hitherto astronomers have not been in the habit of using. I have already made five reviews of the zodiac, without detecting any of these concealed objects. Had they been less resembling the small stars of the heavens, I must have discovered them. But the method which will now be put in practice, will completely obviate all difficulty arising from the asteroidical appearance of these objects; as their motion, and not their appearance, will in future be the mark to which the attention of observers will be directed.

A laudable zeal has induced a set of gentlemen on the

Continent, to form an association for the examination of the zodiac. I hope they will extend their attention, by degrees, to every part of the heavens; and that the honourable distinction which is justly due to the successful investigators of nature, will induce many to join in the meritorious pursuit. As the new method of observing the zodiac has already produced such interesting discoveries, we have reason to believe that a number of asteroids may remain concealed; for, how improbable it would be, that if there were but two, they should have been so near together as almost to force themselves to our notice. But a more extended consideration adds to the probability that many of them may soon be discovered. It is well known that the comas and tails of comets gradually increase in their approach to the sun, and contract again when they retire into the distant regions of space. Hence we have reason to expect, that when comets have been a considerable time in retirement, their comas may subside, if not intirely, at least sufficiently to make them assume the resemblance of stars; that is, to become asteroids, in which state we have a good chance to detect them. It is true that comets soon grow so faint, in retiring from their perihelia, that we lose sight of them; but, if their comas, which are generally of great extent, should be compressed into a space so small as the diameters of our two asteroids, we can hardly entertain a doubt but that they would again become visible with good telescopes. Now, should we see a comet in its aphelion, under the conditions here pointed out, and that there are many which may be in such situations, we have the greatest inducements to believe, it would be a favourable circumstance to lead us to a more perfect knowledge of the nature of comets and their orbits; for instance, the comet of the year 1770, which

Mr. LEXELL has shewn to have moved in an elliptical orbit, such as would make the time of its periodical return only about $5\frac{1}{2}$ years: if this should still remain in our system, which is however doubtful, we ought to look for it under the form of an asteroid.

If these considerations should be admitted, it might be objected, that asteroids were only comets in disguise; but, if we were to allow that comets, asteroids, and even planets, might possibly be the same sort of celestial bodies under different circumstances, the necessary distinction arising from such difference, would fully authorise us to call them by different names.

It is to be hoped that time will soon throw a greater light upon this subject; for which reason, it would be premature to add any other remarks, though many extensive views relating to the solar system might certainly be hinted at.

Additional Observations relating to the Appearances of the Asteroids Ceres and Pallas.

May 4, $12^h\ 40'$. 10-feet reflector; power $516\frac{1}{2}$. I compared Ceres with two fixed stars, which, in the finder, appeared to be of very nearly the same magnitude with the asteroid, and found that its coma exceeds their aberration but in a very small degree.

$12^h\ 50'$. 20-feet reflector; power 477. I viewed Ceres, in order to compare its appearance with regard to haziness, aberration, atmosphere, or coma, whatever we may call it, to the same phenomena of the fixed stars; and found that the coma of the asteroid did not much exceed that of the stars.

I also found, that even the fixed stars differ considerably in this respect among themselves. The smaller they are, the larger in proportion will the attendant haziness shew itself. A star that is scarcely perceptible, becomes a small nebulosity.

10-feet reflector. 13^h 10′. I compared the appearance of Pallas with two equal fixed stars; and found that the coma of this asteroid but very little exceeds the aberration of the stars.

14^h 5′, 20-feet reflector. I viewed Pallas; and, with a magnifying power of 477, its disk was visible. The coma of this asteroid is a little stronger than that which fixed stars of the same size generally have.

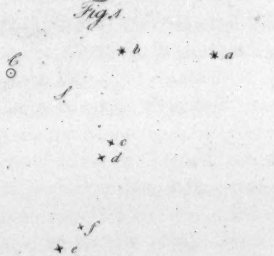


Fig. 1.
b
a
c
f
d
f
e

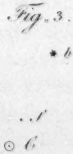


Fig. 3.
b
f
c

Fig. 2.
d
2 f
1 f
c
a
c
d
e
b

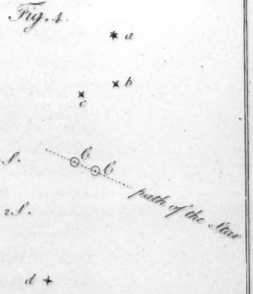


Fig. 4.
a
b
c
1 f.
c
c
path of the Star
2 f.
d

Bauer sc.

IX. *Description of the Corundum Stone, and its Varieties, commonly known by the Names of Oriental Ruby, Sapphire, &c.; with Observations on some other mineral Substances. By the Count de* Bournon, *F. R. S.*

Read March 25, 1802.

WHEN, in the year 1798, I presented to the Royal Society, in conjunction with Mr. GREVILLE, a Paper on the Corundum Stone,* I gave some hints of an opinion which I, as well as Mr. GREVILLE, had already formed, namely, that the said stone was absolutely of the same nature with those stones or gems which mineralogists, following the example of the jewellers, had hitherto distinguished by the epithet *oriental*. This opinion was founded upon circumstances which appeared to me perfectly satisfactory; but these circumstances had not yet been sufficiently examined, nor were they sufficiently striking, to

* See Phil. Trans. for 1798. p. 428. My principal intention, in the Paper here referred to, was, to bring together the various observations which had been then made respecting the stone here treated of. The great number of specimens which have since been successively sent from different parts of the East Indies, have enabled me to form a more correct, and, in some respects, a different opinion of it. I therefore thought it would be of more advantage to science, instead of presenting to the Royal Society a supplement to my former Paper, to collect into one point of view, every information I could obtain upon the subject. I have consequently endeavoured, in the following Paper, to give, as far as I am able, a complete mineralogical history of this stone; my former account being, when compared with this, a very imperfect one.

obviate every possible objection; and, consequently, my opinion was not yet in a state fit to be presented to the Royal Society, as an established truth. Since that time, I have never lost sight of this object, nor have I neglected any means in my power, which could conduce to the end I had in view; and I may say, that my success has far surpassed my expectations. The specimens of corundum that have been lately sent from India, joined to the very considerable collection of oriental gems, in their perfect crystalline forms, which I have been able to procure, have afforded me the most satisfactory demonstration that a mineralogist can wish for; and nothing was now wanting to fix, in a complete and decisive manner, the general opinion respecting this stone, except to give it that additional support which is furnished by chemical investigation. Mr. KLAPROTH indeed had already published an analysis of the corundum stone, and of the sapphire; but he had not submitted to the same scrutiny, the perfect red corundum or oriental ruby; it is possible also, that the specimens of corundum he made use of in his analysis, which had been taken from among the first specimens of this stone sent from India, were not so pure as might have been wished, and that this impurity was the cause of the difference, (which however was very trifling,) between the result of their analysis and that of the sapphire. I therefore chose, from among the specimens of corundum which had been sent from China, from the kingdom of Ava, from the Carnatic, and from the coast of Malabar, such pieces as appeared to me the most pure; and, after having added to them a quantity of oriental rubies and sapphires, sufficient for many repeated analyses, I requested Mr. CHENEVIX, whose chemical labours are so useful to mineralogy, by his constant application

of them to that science, to have the kindness to join with me in the investigation I had undertaken. The Royal Society will perceive, in the detail given by Mr. CHENEVIX himself, of the analyses which he has made, not only of the different varieties of corundum, but also of the substances which accompany this stone in its matrix, how very satisfactory to science are the results of those analyses; insomuch, that I can now offer to the Society, as one of the best established truths, what, in the year 1798, I mentioned merely as a suspicion which had great probability in its favour; and can also, in consequence of the particular study I have made of all the varieties of stones that I have here joined together, under the general denomination of corundum, present to the Society a collection of facts, for the most part unknown, which, altogether, may be considered as forming a mineralogical history of this substance.

Although the epithet oriental has been for a long time used by the lapidaries, to express, in gems or precious stones, a degree of hardness superior to that of other stones, (the diamond excepted,) which made them capable of taking a more brilliant polish; and although, following the example of the lapidaries, naturalists had employed the same term by way of distinguishing them, there still remained a great uncertainty, respecting the nature of the analogy which really existed between the various stones to which the above epithet was applied.

The nomenclature here spoken of was not, at its origin, the result of any mineralogical knowledge; in consequence of which, a number of stones, of a totally different nature, were united together, for no other reason but because, among those of the same colour, some were found to be of a much superior

degree of hardness to others; and, as those which were the hardest most commonly came from the East Indies, all hard gems were called oriental, as a general mark of discrimination. The chief distinguishing character of gems was then derived from their colour, which had caused them to be denominated sapphire, ruby, amethyst, topaz, emerald, chrysolite, &c. and it was thought sufficient to add to these names the epithet oriental, to distinguish those among them whose hardness was superior to that of the others.

ROME' DE LISLE was the first mineralogist who threw a gleam of light, into the obscurity which existed in this confused assemblage of stones. His classification of gems, although it had not yet attained the degree of perfection to which the science of crystallography (of which he had just laid the foundation) may hereafter carry it, was undoubtedly one of the greatest steps mineralogy had made, at the time when the second edition of his work, upon this new character of stones, was published. After having fixed, according to their different characters, and particularly according to that which was derived from their crystalline forms, the place which each of the species composing this particular class of lithology ought to occupy, he placed at the head of them, under the title of *oriental ruby*, all those stones which, being possessed of a degree of hardness superior to that of all others, (except the diamond,) admitted a more brilliant polish, and appeared under the form of a hexaedral pyramid, or of two, joined base to base, the solid angle of whose summit, taken upon two of the opposite faces, varied, according to him, from 20° to 90°. He added also, that this stone presented all sorts of colours, either separately, or united together in the same stone. Nearly at the same time,

Mr. WERNER, following the system his genius had just then formed in mineralogy, was conducted to exactly the same results.

The very small number of perfectly defined crystals of this stone which existed in the cabinets of Europe, (they being much more rich in cut and polished specimens,) did not permit either of the above-mentioned mineralogists to obtain a clear idea of the whole of its characters, so as to enable him to give a proper description of it. ROMÉ DE LISLE, indeed, may be said to have made a step backwards, by excluding from the number of its crystalline forms, the rhomboid, which, in the first edition of his Crystallography, he had assigned to it, on account of a crystal of that form, which was among the stones preserved in the *Garde Meuble* of the King of France. This stone, which was of a blue colour inclining to purple, and of a very considerable size, (since it weighed no less than 132 carats,) had been polished; a circumstance which had necessarily altered its form in some measure, although there is reason to believe that it had been polished only upon its natural surfaces. ROMÉ DE LISLE, however, who had, merely for the above reason, excluded the rhomboid from the forms of the sapphire, being induced afterwards to recur to his former opinion, made another mistake, by assigning to this substance, the rhomboid of sulphate of iron or martial vitriol, (the measures of which are very nearly from 82° to 98°,) as that which properly belonged to it.

Our mineralogical knowledge with respect to corundum, was therefore very little advanced, when we became acquainted with that which was sent from India. Mr. GREVILLE, in the Paper to which I have already referred, has given a very interesting and instructive account, not only respecting the introduction of this

stone into Europe, but also respecting the information which, in consequence of his repeated inquiries, he had been able to obtain with regard to its local situation; and it is chiefly to him that we are indebted, for nearly all the specimens of this stone which exist in the various collections, as well as for the attention which has been paid to it.

From the moment when this stone became known, the opinions which were formed, respecting the place it ought to occupy in mineralogy, were very various; indeed, it was natural they should be so, with regard to a stone which, as yet, was only known by means of a few specimens, (by no means sufficiently numerous to supply every collection,) and whose local situation, as well as every thing else relating to it, was totally unknown. It has suffered, in this respect, the fate usually attendant on things so circumstanced; yet, whatever erroneous notions have hitherto been entertained respecting it, it has at last, I trust, found the place assigned to it by nature and truth.

The progress of chemistry, with respect to this stone, has not been more certain than that of mineralogy. It was first placed among those substances which were considered as composed of new earths; afterwards it was classed among those which were found by analysis to be chiefly, and indeed almost exclusively, composed of argill. This was already a great step towards the knowledge of its real nature; since it was thereby placed, if not by the side of, at least at a very inconsiderable distance from, the oriental gems, then known chiefly by the name of sapphire.

It is, in fact, among those gems or stones, now known by the names of sapphire, oriental ruby, &c. that corundum ought

to be placed; but the progress by which we have arrived at this degree of knowledge was necessarily very slow, and was impeded by continual obstacles: for the scarcity and smallness of the crystals of corundum, and the impression naturally made upon our minds by the various appearances it exhibited to us, were by no means likely to lead us to form a true judgment respecting it. So that Mr. WERNER, whose great and acknowledged talents have justly caused his opinion to be considered, nearly throughout all Germany, as of the highest importance in all mineralogical decisions, has hitherto continued to place corundum between pitchstone and felspar; consequently, he has removed it to a considerable distance from the sapphire, since there exists, according to his classification, nearly thirty intermediate substances.

Crystallography also offers some difficulties with respect to this stone; and these difficulties are only to be guarded against by a very particular study of it, and especially by an accurate examination of all its varieties, as objects of comparison. The Abbé HAUY, to whose great knowledge of crystallography all Europe is eager to do justice, although he gave some indications that he began to waver in his opinion, did not think there were reasons sufficiently strong to adopt that which I had, without satisfactory evidence, advanced in 1798; and has continued to separate the corundum from the sapphire, giving to the latter the name of *Telesie*. In the new System of Mineralogy, which the Abbé HAUY has just published, he places corundum immediately after felspar, and before ceylonite, the name of which he has changed into *Pleonaste*. One cannot help being astonished that the very great hardness of this stone, as well as its great gravity, did not lead him to

place it nearer those stones with which, from their possessing those two qualities, it seemed to have some analogy. Perhaps he was not in possession of specimens of sapphire, or of oriental ruby, or of corundum, sufficiently characterised to serve as objects of comparison; and I cannot help expressing great regret, that the crystals of corundum which were sent to him by Mr. GREVILLE, selected by myself from his superb collection, and to which I had the pleasure of adding an almost equal number from my own, were not sufficient to carry conviction to Mr. HAUY's mind; as it would have given me great satisfaction to find that my observations, upon this interesting substance, perfectly coincided with his. The opinion of a naturalist so justly celebrated as Mr. HAUY, will naturally have great weight in the minds of those who pursue the study of mineralogy; for which reason, after giving a particular description of corundum, comprehending all the characters which are peculiar to it, I shall endeavour to remove every objection which this mineralogist still thinks it right to offer, against its union with the sapphire, oriental ruby, &c.

The substance here treated of, has hitherto presented itself to our notice under two appearances, which differ so much from each other, in the greater number of those characters which most forcibly affect our senses, particularly those which concern the organ of sight, that we cannot be much surprised to find that mineralogists feel some reluctance, at the idea of uniting together substances which appear so very dissimilar.

Under one of these appearances, in which it is known by the name of corundum, this substance presents itself either in fragments, or in crystals of a pretty large size; sometimes, indeed, of a very considerable one. The surface of these crystals is

generally dull and rough; their texture, which is very much lamellated, is shown to be so by their fracture, which is obtained without much difficulty, as the adherence of their crystalline laminæ to each other is not very strong, and is easily overcome; and the crystal or fragment may always be brought to the rhomboid, its primitive form. Their colour, which is most commonly rather dull, is a whitish, greenish, and sometimes yellowish gray. Specimens of a purplish red, or of a blue colour, have always been extremely rare; indeed, a short time since, no such specimens were known, excepting a very few, preserved in the collection of Mr. GREVILLE, and some small fragments he had given away; but the specimens which have been lately sent from the district of Ellor, have contributed to increase their number.

Under the other appearance, (in which this substance is known by the names of sapphire, ruby, &c.) it offers itself, on the contrary, in crystals which are generally of a very small size, and have a smooth and brilliant surface. Their transparency is often very great; and it seldom happens that they are not semi-transparent, in a greater or less degree. They are more difficult to break in the direction of their crystalline laminæ; and this difficulty increases, in proportion to their purity and their brilliancy. Their colours are much more beautiful, more variegated, and more lively.

With respect to the name of this substance, as, in its most common state, it is known in India, (its native country,) by the name of corundum, and as that name has been generally adopted in Europe, I have thought proper to continue it, and shall distinguish, by the terms *perfect* and *imperfect*, the two different states in which it presents itself to our observation. Nothing,

in my opinion, occasions greater obstacles to the progress of a science, than making a change in its nomenclature, especially when that change is made without a general agreement. For, by this means there exists no fixed basis; and, consequently, every one thinks he has a right to exercise an arbitrary power in this respect, and to reject the name given to a substance by those who first observed and described it, for the purpose of giving it one more suitable to his own ideas. And thus, at last, it becomes necessary, (in order that the labours of our predecessors may not be wholly useless,) to fill the new works on the subject with a tedious list of synonyms, which too often becomes in the end a mass of uncertainty, and a subject of everlasting discussion.

COLOUR.

Although the colour of stones, strictly speaking, may be considered as a very variable circumstance, and as one which can by no means be included among those fixed characters which determine the nature of the stone, it is nevertheless certain, that many stones seem disposed to assume some colours in preference to others; and, therefore, the colour of a stone, though an uncertain character, may sometimes serve as a secondary mark of distinction; particularly, if we are cautious not to draw any inferences from it, except in conjunction with other characters. As its chief use is, to fix the value of precious stones, and as, in those here treated of, it has served as a basis for the former classification of them, it becomes more necessary to give a minute description of it in this substance than in any other.

I have already said, that the colour of common corundum,

(which I shall in future distinguish by the name of *imperfect* corundum,) has, in general, very little brilliancy; but, in proportion as the crystals announce, by their greater transparency, a greater degree of purity and perfection, their colour becomes more lively and more brilliant; this, however, seldom happens, except in crystals of a small size. The colour of these crystals is various, and seems to depend very much upon the place where they are found. In the Carnatic, the prevailing colour is a grayish white; which, however, very often approaches to a pale green, and sometimes to a yellowish cast. They are also found, but much more rarely, of a red, and of a blue colour; and, when they are of those colours, the red always inclines to the purple, and the blue is of that azure kind which is generally known by the name of sapphire blue. In the corundum of China, and in that of the kingdom of Ava, the colour is generally a green, more or less deep, with a dull appearance; or it is brown. The corundum of the coast of Malabar, appears of a reddish brown in those parts which are opaque; but, whenever there is, in any part of it, the smallest degree of transparency, the forementioned colour always appears to be accompanied by a tinge of purple.

In the perfect corundum, which is found in Pegu and in Ceylon, but which is now most commonly brought (when in its natural or unpolished state) from the last mentioned place, the colours are much more various, and more lively. The chief of these colours are, red, blue, and yellow. The red colour constitutes the stone known by the name of oriental ruby; but it seldom happens that this colour has not a small mixture of blue, which gives it a tinge slightly inclining to purple. The blue colour is always that which is known by the name of azure

blue; and the stone which possesses this colour is distinguished by the name of sapphire. The yellow colour is seldom pure, being in general more or less mixed with a reddish tint. The oriental gem of this colour is called the oriental topaz. From a duly proportioned mixture of the blue and the red, is produced the purple colour, which constitutes the oriental amethyst. Sometimes the red colour is predominant, at other times the blue; and, in the latter case, the stone possesses that beautiful purple colour which is so pleasing to the eye. Stones of this colour are among the most rare of those belonging to this substance. By the union of the blue colour with the yellow, is formed the green, which produces the oriental emerald; but there is usually mixed with this colour a small proportion of red, which gives to the green a brown and rather dull tinge. Sometimes however the yellow colour is predominant, which of course gives the green a yellowish cast, and then the stone becomes the oriental chrysolite. I have not yet seen any of the green stones, or oriental emeralds, in which the green colour was perfectly pure and brilliant, as it appears in the true emerald, called the peruvian one. In the mixtures of which I have just spoken, the colours are, in general, perfectly blended together; sometimes however they exist in a separate state, and so distinctly, in the same stone, that the mixed colour is only perceptible at the point where the different colours meet. At other times, these colours being only coarsely mixed, and not blended together, the stone presents the one or the other of them more distinctly, according to the position in which it is held.

TRANSPARENCY.

The crystals of corundum from the Carnatic, having their

surface always rough, and being usually more or less impregnated with fine particles of the various substances which compose their matrix, very seldom possess any degree of transparency; but, when these crystals are broken, their fragments generally have a degree of semi-transparency, but most commonly a very slight one, unless the fragments happen to be very thin; even then, I have never found them perfectly transparent.

If such of these fragments as have the greatest degree of semi-transparency, are held between the eye and the light, there may be observed, within their substance, a great number of lines or fissures, which cross each other, and prevent the free passage of the light, the greater part of which is reflected. These fissures, which arise from there not being a complete adherence between all the parts of the crystalline laminæ, are the principal cause of the slight degree of transparency commonly met with in the kind of corundum here spoken of; which kind may truly be said not to have attained, in its crystallization, all the perfection it is capable of acquiring, and which may be observed in the perfect corundum of Ceylon.

I think it also right to observe, that the corundum of the Carnatic, when of a red or a blue colour, has always a greater degree of transparency, and is more pure, than that which is of any other colour; and, in these respects, the corundum of a blue colour is much superior to that which is red.

In the imperfect corundum of China and of Malabar, although the surface of the crystals is also generally rough, yet, as they are less impregnated with foreign substances, it is not uncommon to observe in them a greater or less degree of transparency at their edges. Some crystals have, indeed, been sent to us from China, (very small ones, I confess, but very perfect,) which

possessed a degree of transparency very little inferior to that of the perfect corundum of Ceylon. The terminal faces of the crystals from the two last mentioned places, are very frequently what is called *chatoyant*; a property of which I shall hereafter speak more particularly.

The perfect corundum of Ceylon, whatever may be its colour, always has a greater or less degree of semi-transparency; and very often is perfectly transparent. Sometimes, indeed, the crossed fissures already spoken of, as existing in the imperfect corundum, are also to be observed in the interior part of this; but, when that is the case, they are less strong, and less numerous. The crystals of the perfect corundum have a smooth and brilliant surface; and they show all the transparency their substance possesses, without its being necessary, as in the imperfect corundum, to break them for that purpose. In general, when they have an inferior degree of transparency, whatever their colour may be, their terminal surfaces possess the appearance called *chatoyant*, which, as I have already said, is very frequently observed in the corundum of China, and in that of the coast of Malabar.

In general, although the perfect corundum of a blue colour, or sapphire, has exactly the same characters as that which is of a different colour, it appears to me certain, if I may judge from the great number of specimens I have seen, that it more commonly possesses a perfect transparency, than that which is of any other colour. I have already made a similar observation, in speaking of the imperfect corundum of the Carnatic. To this circumstance must be attributed, the superior value of an oriental ruby, if without defect and of a certain size, when compared with that of a sapphire of equal size and equally

perfect. To the same cause must also be ascribed, the scarcity of fragments of sapphires, in comparison with those of rubies, in the sand of Ceylon which has passed through the hands of the lapidaries; the fragments of the former being usually more transparent, they are selected from it, as more worthy to be cut and polished.

HARDNESS.

Corundum is, next to the diamond, the hardest of all stones; but, with respect to this character, the degrees of intensity are various; and this variety depends principally upon the degree of purity, and the colour, of the stone.

When the imperfect corundum of the Carnatic is neither of a blue nor of a red colour, its hardness is less considerable, in proportion as its transparency is less, and its internal substance more full of those lines or fissures, which, as I have already said, are commonly observed in it. Such corundum may be scratched by that which is more transparent, though of the same colour. The latter, (supposing the degree of purity to be nearly equal,) may in its turn be scratched by that which is of a purplish red; and this last, by the corundum of a blue colour; which is the hardest of all those varieties of this stone that I have distinguished by the name of imperfect corundum. The hardness of the imperfect corundum of China, and of that from the coast of Malabar, appear to be equal. This hardness, which is rather inferior to that of the blue corundum of the Carnatic, is however somewhat greater than that of the other varieties.

The perfect corundum of Ceylon of a red colour, or oriental ruby, the hardness of which seems to be nearly the same as that of the imperfect blue corundum, is superior in hardness

to all the other varieties of the latter kind. In the perfect corundum of other colours, the hardness is nearly the same as in the red; that which is of a blue colour, or sapphire, and only that, rather exceeds the others in hardness. We have just seen, that in the imperfect corundum also, the blue colour was accompanied by a degree of hardness greater than that of the other colours.

This substance emits pretty bright sparks, when struck with a piece of steel; but they are by no means proportioned to its hardness. If a piece of flint be struck with the same force, the sparks it produces are more numerous, as well as more bright; and it is possible to obtain sparks from flint, by a very slight blow, such as would not be sufficient to produce them from perfect corundum. It is also necessary, in order to obtain sparks from corundum, that the stone should have pretty sharp edges: if the part that is struck is obtuse, it is with some difficulty that any sparks can be obtained. The imperfect corundum, however, has, in this respect, some advantage over the perfect kind.

PHOSPHORESCENCE.

The substance here treated of becomes, like quartz, phosphorescent by collision; it requires only, in order to exhibit this property, a somewhat stronger degree of friction. The light which it emits has also less intensity; and does not appear to be accompanied by the smell which is peculiar to that obtained from quartz. A very remarkable circumstance may likewise be observed respecting this light. In all the varieties of this stone which are of a red colour, whether of the imperfect or of the perfect kind, or oriental ruby, the light here spoken of is of a very deep fire colour, similar to that of red hot iron, when

heated to the degree known by the term *cherry red*. The sparks which are obtained from this stone by means of a piece of steel, have also some appearance of the above colour. These phenomena may perhaps serve to assist us in acquiring further knowledge respecting the cause of the phosphorescence of stones, of which we have hitherto had no very satisfactory explanation.

GRAVITY.

The specific gravity of corundum, in its different varieties, presents a series of interesting facts, particularly when they are compared with what has been already observed with respect to its different degrees of hardness. The great interest I have felt in the study of this substance, has caused me to take particular care in the examination of such of its properties as might lead to a perfect knowledge of it. I will now state the results of the observations with which the character now treated of has furnished me.

Of 33 specimens of the different varieties of imperfect corundum, the mean specific gravity was 3931. The lightest was 3875; and the heaviest 3981. Six of the 33 were above 3900. Eleven were between 3900 and 3931; and the remaining sixteen were above 3931, which, as I have already stated, was the mean proportion.

The mean specific gravity of the perfect red corundum, as determined by 20 specimens of oriental ruby, was 3977. The lightest of these was 3933. Five of the specimens were above 4000. One alone was as high as 4087; it was of a deep red colour, was perfectly transparent, and had been cut.

Sixteen different specimens of sapphire, gave a mean specific gravity of 4016. The lightest was 3907; it had scarcely any

colour, and was nearly opaque. The heaviest was as high as 4161; this was of a beautiful deep blue colour, and was very transparent. Three of the 16 were above 4100.

The inferences which I think myself warranted to draw from the results of the above-mentioned trials, are,

1. That the specific gravity of the imperfect corundum is always less considerable than that of the perfect kind.

2. That this gravity varies according to the degree of perfection of the crystallization; and, consequently, according as the stone is more or less transparent.

3. That, in general, the corundum of a blue colour, whether of the perfect or the imperfect kind, is of a greater specific gravity than that of any other colour.

What is here stated respecting the specific gravity of the different kinds of corundum, is exactly analogous to what has been already mentioned respecting their various degrees of hardness.

CRYSTALLINE FORMS.

The primitive form of corundum, whatever may be its degree of perfection, is a rhomboid slightly acute; the obtuse angles of the planes measuring 94°, and the acute ones 86°. (See Plate VI. Fig. 1.) The description of the crystalline forms will be more easily and more clearly understood, by considering (as I shall constantly do in what follows) this rhomboid as being formed by the union of two triedral pyramids, united at their bases; the solid angle of the summit will then be formed by the meeting of three of the more acute angles; and its measure, taken upon one of its edges, and in the middle of the opposite face, will be very nearly 95° 30′.

Whatever the form of the crystals of this substance are, they may always, by dividing them, be ultimately brought to the rhomboid here spoken of; and, when they are broken, such of the fragments as are made in the direction of the laminæ, very often present the same rhomboid, in a very regular form. Indeed, it is the only method of obtaining this crystal, in the imperfect corundum; for, among all the crystals of that kind of corundum which have been sent from the East Indies, not one has yet presented its primitive form. With respect to the perfect corundum, I have been more fortunate; as, besides several fragments which exhibited this rhomboid very exactly, I have found four of these primitive crystals perfectly defined. One of them is a sapphire, and is in the collection of Sir JOHN ST. AUBYN; the three others are oriental rubies, and are in the collection of Mr. GREVILLE.

First Modification. The summit of the pyramid, (as very frequently happens in calcareous spar, and in most of the stones which have a rhomboid for their primitive form,) is often replaced by a plane which is perpendicular to the axis. This plane then makes, with those of the rhomboid, an angle which differs very little from 129° 30′; and, as the extent of the plane is more or less considerable, it often causes great difference in the appearance of the crystals. Sometimes it does not descend so low upon the faces of the rhomboid, as to reach their small diagonal. (Fig. 2.) At other times, it exactly reaches to the diagonal. (Fig. 3.) And, very often, it descends more or less below it. (Fig. 4). This last variety is frequently met with in the perfect red corundum, or oriental ruby. I also know four instances of this form in the sapphire. The variety shown in Fig. 3 is rather scarce; but that of Fig. 2 is the most rare of the

whole. I have likewise observed the two last, among some small crystals of imperfect corundum from China, which were pretty transparent.

Second Modification. At other times, the edges of the base of the primitive rhomboid are each of them replaced by a single plane, which is parallel to the axis, and which, when its extent is rather considerable, separates the two pyramids by a hexaedral prism with rhombic planes. I have never seen this modification with complete pyramids, as it is represented in Fig. 5, but I have often observed it combined with the preceding modification. This combination is not unfrequently met with in the oriental ruby, in which, the two varieties represented in Figs. 3 and 4 are found with a small beginning of a prism, as is shewn in Figs. 6 and 7. There are also in the collection of Sir JOHN ST. AUBYN, two crystals of sapphire, belonging to the same variety, one of which is tolerably regular in its form; but it is much more common to find these crystals with prisms of rather greater length, as is represented in Figs. 8 and 9. In Mr. GREVILLE's collection also, there is contained a crystal of a pretty large size, and very perfect, in which the plane that has replaced the solid angle of the summit of the pyramid is very small, as in Fig. 10. All these varieties, but particularly those represented in Figs. 8 and 9, are likewise found among the small transparent crystals of imperfect corundum brought from China.

When the decrease produced by the plane which has replaced the solid angle of the summit of the rhomboid, has begun to take place nearly at the same time with, or even previous to, that which gives rise to the planes which replace the edges of the base, (as is indicated by the length of the sides of the

prism,) it often happens that there remains no trace of the planes of the primitive rhomboid: the crystal is then a regular hexaedral prism. (Fig. 11.) This variety, which is very common in the perfect corundum of a red or of a blue colour, is also common in the imperfect kind; it is indeed, in certain districts, particularly in the Carnatic, almost the only form that is met with. In all these crystals, the prism here spoken of differs considerably in its length; sometimes it is very much elongated; at other times it is very short, as is represented in Fig. 12.

Third Modification. The primitive rhomboid is frequently observed to have undergone, in its crystalline laminæ, a decrease at those flat angles which rest upon the common base. This decrease occasions, in each of the pyramids of the rhomboid, six new planes equally inclined, which thereby render the pyramids enneaedral, (as is seen in Fig. 20,) and which, when this modification is perfect, (that is to say, when the planes belonging to it have destroyed every trace of the primitive rhomboid,) change the crystal into a dodecaedron, formed by the union, base to base, of two hexaedral pyramids with isosceles triangular faces, as in Fig. 13. At present, I shall only take notice of the pyramidal form of these crystals, without paying any attention to the inclination of the faces of the pyramids; for we shall see, at the end of this modification, that the decrease which occasions it is subject to considerable variation, changing, at the same time, the inclination the faces of the pyramids have to each other.

It very rarely happens that we find this dodecaedron perfectly complete, that is to say, with each of its pyramids terminating in a single point, by the exact meeting of all its faces. I know only

one instance of this form, which I met with in a small sapphire, that I have placed in the collection of Mr. GREVILLE. There are indeed two specimens, nearly similar to the above, in the collection of Sir JOHN ST. AUBYN; but two of the opposite faces of their pyramids have increased to a greater degree than the others, which renders them cuneiform.

It is much more common to find the crystals of this modification combined with the first, and consequently having the solid angle of their summits replaced by a plane. Sometimes this new plane is very small, as is shown in Fig. 14. At other times, it is more considerable, as in Fig. 15. The above varieties are less common in the red perfect corundum, or oriental ruby, than in the blue perfect corundum, or sapphire, of which it is the most usual crystalline form, and in which, the plane that has replaced the summits of the pyramids is frequently very small. These varieties are likewise often found among the crystals of imperfect corundum of China; but it is very rare, on account of the irregularity of their surface, to meet with them perfectly defined. They are met with in a much more perfect state, among the crystals from the coast of Malabar; some of these indeed are so perfect, that, were it not for their reddish colour, they would certainly be taken for very beautiful sapphires. One of these crystals, which is in Mr. GREVILLE'S collection, is more than an inch in length. Another, which is cuneiform, and has one of its pyramids broken, is above two inches long. In the crystals of imperfect corundum from the Carnatic, I have never met with any thing more than very slight traces or elements of this pyramidal form.

There frequently remain upon the crystals belonging to these varieties, particularly when the terminal faces are of a pretty

considerable size, more or less evident traces of the planes of the primitive rhomboid; as appears by small isosceles triangular planes, of greater or less extent, situated upon three of the alternate solid angles, formed by the meeting of the terminal faces with those of the pyramid. (Fig. 16.)

It very often happens, in this modification, that the plane which has replaced the solid angle of the summit, acquires a more considerable increase in one of the pyramids than in the other; and indeed, most commonly, this increase is such as to cause the pyramid entirely to disappear. The crystal then becomes a simple hexaedral pyramid, which is either complete, as in Fig. 17, Plate VII. (but this very rarely happens,) or has its summit more or less replaced. (Fig. 18, A.) This variety, which is very common in the crystals of perfect corundum, is also frequently met with in those of the imperfect corundum from China; and it is very usual to see, upon the solid angles of its terminal faces, small isosceles triangles, which are occasioned by the preservation of some parts of the planes of the primitive rhomboid; (Fig. 19.) but they are seldom so regular in their form as they are represented in the figure.

I have often seen small crystals of oriental ruby that exhibited a very pretty variety, as they showed, at the same time, the primitive rhomboid with its summit strongly replaced, and the incipient change to the form of the hexaedral pyramid which constitutes this third modification: this variety is represented in Fig. 20. There are, in Mr. GREVILLE's collection, two very perfect crystals of this form.

The second modification, that in which the pyramids of the primitive rhomboid are separated by an intermediate hexaedral prism, is often combined with the abovementioned union of

the first and third modifications. There exists, for example, in Mr. GREVILLE's collection, an oriental ruby, which exhibits the variety shown in Fig. 20, with the rudiments of an intermediate prism, as is seen in Fig. 21. This variety is also sometimes found among the small transparent crystals of imperfect corundum from China.

In four other crystals, also in Mr. GREVILLE's collection, the prism is very much elongated; and the plane which has replaced the solid angle of the summit of the pyramid is much more extended, as in Fig. 22. These crystals, which are oriental rubies, are in perfect preservation at their two extremities.

There is besides, in the same collection, another crystal, also an oriental ruby, which differs from the preceding, in having no traces left of the planes of the primitive rhomboid. The crystal, consequently, appears to be a regular hexaedral prism, with the edges of its terminal faces bevelled. (Fig. 23.)

In five others, the pyramid has made more progress; and, in all of them are to be seen, on their terminal faces, some slight traces of the primitive rhomboid. (Fig. 24.)

Lastly, in one other specimen, the pyramid is nearly complete, as in Fig. 25. I also know two sapphires, which exhibit an intermediate variety, between the two last-mentioned forms.

One of the most striking characters of corundum is, the great variety exhibited by this pyramidal modification, in the inclination of the faces of the pyramids to the axis of the crystal, and, consequently, in the more or less rapid decrease that has taken place in the crystalline laminæ, at the plane angles situated on the common base of the two pyramids which compose the primitive rhomboid. Among the crystals of imperfect corundum, from the different districts in which this substance

has been hitherto found, which form part of Mr. GREVILLE's collection, and are sufficiently perfect to admit of being measured with accuracy, there is one, of which the solid angle at the summit, taken in the middle of two of the opposite pyramidal faces, is 50°; two of 40°; two of 35°; nine of 24°; and seven of 12°. Among the pyramidal crystals of oriental ruby are, one of 50°; one of 40°; four of 30°; one of 24°; and four of 12°. In the sapphire there are, one of 50°; two of 40°; one of 35°; two of 30°; one of 24°; and two of 12°. If to these measures we add those of two sapphires, and of two oriental rubies, in the collection of Sir JOHN ST. AUBYN, we shall also have 58° and 20°; and we may consequently state, from our present knowledge respecting this substance, that it admits no less than eight different decrements of the laminæ, at the same angle of the base; each of which produces a pyramidal modification. And the measure of the solid angle of their summits, (considering the pyramids as complete, and supposing at the same time that the very great care I have taken has prevented me from committing any error,) are 58°, 50°, 40°, 35°, 30°, 24°, 20°, and 12°.*

This difference in the inclination of the faces of the pyramids, in the corundum of a pyramidal form, often appears in a very striking manner in the same crystal. I have frequently met with oriental rubies, and also with sapphires, in which the faces of the pyramids, after having for some time preserved a certain degree of inclination, evidently appeared to have changed it, in

* In Figs. 18 A, 18 B, and 18 C, are represented this simple pyramidal modification, having 58°, 35°, and 12°, for the measures of the solid angle of the summit of the pyramid: from these figures, it will be easy to form an idea of the appearance of these crystals which have the other measures above enumerated.

order to assume another; this change caused the crystal to terminate by a pyramid less sharp; and, in many instances, it was evident that it had happened several times successively. These variations do not always take place in a regular order in the same crystal; for it very often happens, that some of the faces have undergone two, three, or even four changes of inclination, while others have not undergone so many; and sometimes, indeed, have not undergone any at all. I have seen some of these crystals, of which the irregularity was such that, upon some of the faces, the degree of inclination was changed from a greater to a less; a circumstance which necessarily formed a depressed angle, and thereby produced a very irregular and even deformed shape, in the crystal itself. Among the very small number of crystals from the Carnatic which shew any disposition to assume the pyramidal form, I particularly observed one, in which this irregularity in the mode of decrease is very remarkable. This crystal, on three of its adjacent sides, appears to be a regular hexaedral prism; but, from nearly the middle of two others, also adjacent, it becomes pyramidal, and of that modification in which the solid angle of the summit is of 50°; and, from about one-third of the remaining side, it also assumes a pyramidal inclination, but of that modification in which the solid angle of the summit is of 40°. This crystal, which is represented in Fig. 26, is preserved in Mr. GREVILLE's collection. These pyramidal modifications also very frequently demonstrate, by the great number of transverse striæ which are on their faces, and which sometimes resemble the steps of a staircase, the irregularity with which their decrements have taken place.

Fourth Modification. The primitive rhomboid sometimes undergoes, in those acute angles which contribute to the formation

of the solid angle of the summit, a decrease much more rapid than that we have already mentioned, when speaking of the first modification. This decrease replaces the solid angle by three new planes; which planes, if they were to become of such extent as to cause the primitive faces of the rhomboid to disappear, would occasion a secondary obtuse rhomboid, that would have considerable analogy, in the measure of its angles, with that rhomboid of calcareous spar which is called *lenticular;* that is to say, the solid angle of its summit would measure about 139°; and the plane angles of its rhombs 114° and 66°. I have not yet met with this rhomboid perfectly formed; but it exists, or at least one of its halves, in a very well defined state, at the summit of a simple pyramid, eight or nine lines in height, the solid angle of which summit measures 12°; it is represented in Fig. 27. The great number of striæ, parallel to the small diagonals of the primitive rhombic planes, with which the faces of the secondary rhomboid are covered, prevent me from being perfectly certain respecting the accuracy of the measures I have just stated; but, if they are not strictly exact, they must at least be very nearly so. The crystal I have just described is from the coast of Malabar, and is in Mr. GREVILLE's collection. The planes of the secondary rhomboid are slightly *chatoyant*.

Fifth Modification. Another mode of decrease, of a similar kind, but still more rapid, sometimes takes place at the same solid angle of the summit of the primitive rhomboid. The triedral pyramid which replaces this angle, is then much less elevated than in the preceding modification. When it is complete, that is to say, when there remains no trace of the planes of the primitive rhomboid, the crystal becomes changed into a

new rhomboid, which is much more obtuse than the former one. (Fig. 28.) The rhombic planes have 117°, for the measure of their obtuse angles; and 63°, for the measure of their acute ones. The solid angle of the summit of the pyramid is very nearly 150° 30′; consequently, the angle formed by the meeting of the bases is about 29° 30′.*

There are, in Mr. GREVILLE'S collection, two oriental rubies which exhibit this rhomboid completely formed; its planes are deeply striated, in the direction of the decrease; a circumstance which is very common in all planes that are the result of a rapid decrease, or in which the edges of the laminæ last deposited, deviate considerably from the edges of those which were already formed.

There are also, in the same collection, two perfect hexaedral prisms of corundum from the Carnatic, in which this modification shows itself by small isosceles triangular planes, situated upon three of the alternate solid angles of each extremity. (Fig. 29.) These planes may easily be distinguished

* After having, in this substance, met with a secondary rhomboid that exactly agrees with one of those belonging to calcareous spar, (although the planes which produce it are differently situated upon the primitive crystal,) it appeared to me very extraordinary to meet with a second, which had exactly the same proportions as another of the obtuse rhomboids of the abovementioned substance. In fact, there exists in calcareous spar, a rhomboid much more obtuse than that which ROMÉ DE LISLE named *lenticular*, (called *equiaxe* by the Abbé HAUY,) of which the measures are exactly the same as those which have just been assigned to the rhomboid of corundum; but there is the following difference between them, viz. in calcareous spar, this rhomboid is the result of a decrease along the edges of the pyramid belonging to the primitive rhomboid; whereas, in corundum, it is the result of a decrease at the angles which contribute to the formation of the solid angle of the summit. This modification of calcareous spar has not yet been described; but, indeed, the same thing may be said of many other modifications of that substance.

from those belonging to the primitive rhomboid: first, by their inclination, which is very different, as they make, at their meeting with the edges of the prism, an angle of 110°, whereas the others make an angle of 147° 30′. Secondly, they are usually very deeply striated; a circumstance which rarely occurs in the others. Of the two crystals I have just described, one is nine lines in diameter, and six lines in height; it is also slightly transparent at the edges. The other is much smaller, more transparent, and of a purplish red colour, but rather pale. It is one of the purest specimens of imperfect corundum, particularly of that from the Carnatic, I have ever seen.

There are frequently observed, in the small prisms of imperfect corundum, some traces of the planes above described; they may in general be easily known by their striæ. I have also seen crystals in which were united, at the same time, traces of the two secondary rhomboids of the fourth and fifth modifications, in the manner represented in Fig. 30.

Sixth Modification. There also appears to exist, in this substance, a third rhomboid, which is much more obtuse than either of the two preceding ones; at least it is only to such a modification that I can refer several crystals, both prismatic and pyramidal, of imperfect corundum, which made part of a parcel lately sent to Mr. Greville, from the district of Ellore, in the northern part of the government of Madras. Among these crystals are many hexaedral prisms, of a perfectly regular form, which have their terminal faces inclined in a contrary direction, so as always to make, upon the edges of the prism on which they incline, angles of 100° and 80°. (Fig. 31. Plate VIII.) These terminal faces appear to me to belong to a very obtuse rhomboid, of which, the acute angles of the rhombic planes would

be 60° 46′; the obtuse ones 119° 14′; and the solid angle of the summit 165°. The crystal I have just described, would then be nothing more than the prismatic modification, combined with that which occasions this rhomboid; at both extremities of which, one of the faces of each of the obtuse triedral pyramids, belonging to the new rhomboid, would have acquired (in a contrary direction with respect to its extremities) such an increase as would cause the other faces to disappear. These two faces, having now become the terminal ones of the hexaedral prism, would in fact make, with those edges of the prism on which they would incline, angles of 100° and 80°. This very obtuse rhomboid would be the result of a decrease analogous to the two preceding ones, but still more rapid. Many pyramidal crystals of this kind of corundum, present such inclined terminal faces; but with a difference, in the measure of their angles, conformable to the inclination of the edges of the pyramids.

Seventh Modification. The primitive rhomboid of this substance also undergoes sometimes, though very rarely, a decrease at those acute angles which rest upon the base; and this decrease is such, that it replaces each of the solid angles of this same base, by a plane which is parallel to the axis of the rhomboid. If this modification were complete, it would give rise to a regular hexaedral prism, which would differ from the prism of the second modification, in having its sides corresponding with the solid angles of the base of the rhomboid; whereas the sides of the other correspond with the edges of the said base. I know this modification only by a single crystal, which is in the collection of Mr. GREVILLE; in it is combined the modification here spoken of with the three first. This crystal, which is

of perfect red corundum, or oriental ruby, is almost exactly similar to that represented in Fig. 22, and indeed only differs from it by the prism being dodecaedral, as in Fig. 32.

Eighth Modification. I am also acquainted with this modification only by a single crystal. This crystal, which is a sapphire of a beautiful deep blue colour, is likewise in Mr. GREVILLE's collection. Its form is a simple hexaedral pyramid, which is almost complete, and has 24° for the measure of the solid angle of its summit. Each of its six edges are replaced by a very narrow plane, which is equally inclined upon the two faces that are adjacent to it. This renders the pyramid dodecaedral, with broad and narrow faces alternately, as in Fig. 33. Three of these new planes appear to me to be occasioned by a decrease, which has taken place at the obtuse plane angles that rest upon the base of the rhomboid, but which differs from those which occasion pyramidal modifications, and is of such a nature that (the new planes to which it gives rise being in pairs, and on the same level,) each of the solid angles of the base is replaced only by a single plane. The three others appear to me to be caused by a decrease at the acute plane angles that rest upon the base; but this decrease differs from that of the seventh modification, in being more rapid, and in having the planes to which it gives rise inclined upon the axis of the crystal. The three latter planes have the following peculiarity, viz. their inclination is exactly equal to that of the three others; so that, if the two modifications which are united together in this crystal were complete and separate, they would produce two acute rhomboids, perfectly similar to each other.

FRACTURE AND TEXTURE.

I have already observed, that all the stones which compose the various kinds of this substance, to which I have given the general name of corundum, have a lamellated texture, in a direction parallel to the faces of a rhomboid of 96° and 84°; and also, that they break in a direction parallel to the said faces.

The blue variety of perfect corundum, or sapphire, follows the above law, as well as all the other varieties. It is true, however, as I have already had occasion to mention, that the ease with which the crystals of this substance may be divided, is very various; but observation shows, at the same time, that these variations are governed, in the first place, by the degree of force existing in the attraction of the molecules which compose the crystals, as well as by the perfect adhesion of the crystalline laminæ (composed of these molecules) at all points of their surface; two facts, the existence of which is shown by the difference in the degrees of hardness and transparency of this stone, and which appears to be very considerable. In the second place, the variations here spoken of seem also to depend very much upon the colour these stones possess; for, as I have already observed, they must be governed by the force of attraction, which, in my opinion, varies with the colour. This force appears to exist in the highest degree, in the perfect corundum of a blue colour, or sapphire; it being with great difficulty that this kind of corundum can be broken, in the direction of its laminæ, in such a manner that its fracture shall present that even surface, and that kind of gloss, which fractures made in the above direction generally exhibit. It may be broken with equal ease in any other direction; for instance, in a direction

perpendicular to the axis of the crystal; but, in this last case, the fractures by no means possess such characters as might cause them to be taken for fractures made in the direction of the laminæ; they are always unequal, and partially conchoid. I will even confess, that I have not yet succeeded in breaking a sapphire, according to the direction of its laminæ, in a satisfactory manner. But that which art is not able to perform, is executed by nature: for, besides such sapphires as, upon their terminal faces, retain complete traces of the planes of the primitive rhomboid, I have frequently met with sapphires, both of the prismatic modification and the pyramidal one, in which there were, upon the said faces, one or more fractures, made exactly in the direction of the laminæ; and it was necessary to examine them with great attention, in order not to mistake them for true planes, representing those of the primitive rhomboid.* This kind of fracture is obtained with greater ease in the perfect red corundum, or oriental ruby; and still more easily, in the imperfect corundum. The latter presents, in this respect, a less degree of resistance, in proportion as it is less transparent, and has less colour. This character, however, is subject to great variation: there exist some specimens of this stone, in which such fractures as are here described may be made almost as easily as in calcareous spar; whereas, in others, they are obtained with much more difficulty. I have even seen some pieces which

* I have placed several of these crystals in Mr. Greville's collection, and also in that of Sir John St. Aubyn, and in that of Sir Abraham Hume. The owners of these collections have confided to me the care and arrangement of them, with a degree of liberality which gives me every advantage that could be derived from the absolute possession of them, and consequently diminishes my regret for the loss of my own. I feel too sensibly these advantages, and many others resulting from their friendship and society, not to embrace with pleasure this opportunity of testifying my gratitude.

might be broken, with almost as much ease, in a direction contrary to that of the laminæ, as in the direction of the laminæ; but it most frequently happens, in this case, that the fracture, although made in the natural direction, has not the evenness such fractures usually have, but presents some irregularities, and likewise some conchoidal parts: this remark, however, applies only to such pieces as approach nearly to perfection, with respect to transparency.

There may frequently be observed, in these stones, a character which serves to confirm what I have said respecting the imperfection sometimes observed in their crystallization, which appears to me to arise principally from a want of absolute contact between all the parts of their crystalline laminæ. When some of the faces of the crystals correspond to those of the primitive rhomboid, whether these faces are natural ones or are produced by fracture, the edges of the crystalline laminæ are shown upon them, and sometimes very plainly, by lines which cross each other, in such a manner as to form rhombs of 96° and 84°. This character even becomes of great use in this substance, as it serves to distinguish, in fragments, (which are generally of hexaedral prisms, that being the most common form,) those faces which are occasioned by fracture, from those which correspond to the terminal faces of the prism. These last, also, frequently exhibit lines, which are likewise caused by the edges of the crystalline laminæ; but, as they extend to three only of the alternate angles of the terminal hexagonal face, they trace on it, by crossing each other, either equilateral triangles, or rhombs of 60° and 120°. Figs. 34, A, and 34, B, represent these two different appearances; the first upon the planes of the rhomboid; the second upon the terminal faces.

As it is by no means uncommon, in corundum, (in the same manner as is observed in the beryl,) to meet with elongated prisms, formed merely by the connection or contact of several prisms at their terminal faces, it frequently happens that these prisms, after being separated from each other, exhibit, upon the terminal faces which were in contact, a polish or lustre that might easily cause those faces to be taken for fractures, in a direction perpendicular to the axis. But this appearance is an illusion we must guard against: for, if we endeavour to make any fractures at the extremities of these crystals, they will take place, as usual, upon three of the alternate solid angles; and we shall find it impossible to succeed in making any fractures perpendicular to the axis, except such as are extremely irregular, and exhibit an appearance very different from that exhibited by natural ones. It sometimes happens also, that, by means of the above connection, as well as by some causes of compression, which must necessarily have been frequent with respect to crystals inclosed in their matrix, in the manner those of felspar are inclosed in granite or porphyry, that the terminal faces have varied from their natural position, and have assumed another, which inclines more or less upon the sides of the prism. We must, however, distinguish these accidental varieties, from those crystals in which such an inclination really belongs to the mode of crystallization, and which I have already described, in speaking of the sixth modification. In this latter case, the inclination of the terminal faces is constantly the same; whereas, in the accidental case here treated of, it varies considerably.

There exists also, in this substance, and even among the same crystals, (when hexaedral prisms,) not only of imperfect

corundum but likewise of the perfect kind, of all colours, another accidental variety, which is particularly met with when their irregularity and their opacity announce a want of perfection in their crystallization. Sometimes the edges of the crystalline laminæ may be perceived upon their terminal faces; and, there being more or less distance between them, they exhibit very much the appearance of an irregularity, or a kind of disturbance, in those laminæ which seem to have been deposited upon these faces, and in a direction parallel to them. But, with a little attention, we may perceive that these laminæ, the edges of which are in the direction of three of the alternate solid angles of this extremity of the crystal, can only belong to the laminæ deposited upon the faces of the primitive rhomboid; and, we are very often able, at the same time, to discover their degree of inclination.

A third circumstance attending these crystals, and one which it is more difficult to explain, consists in the appearance of concentric hexagons, parallel to the hexagon formed by the exterior edges of the crystal. These hexagons may sometimes be observed upon the terminal faces, as is shewn in Fig. 35. Their edges have a degree of thickness very perceptible by the eye; and may besides be frequently distinguished from each other, by a difference in their transparency, and sometimes also by a greater or less intensity in their colour. There are preserved, in Mr. GREVILLE'S collection, amongst a pretty large number of crystals in which this circumstance has taken place, two crystals of imperfect corundum from the coast of Malabar, that exhibit it in a very striking manner. In the first of them, one only of these hexagons, placed at nearly an equal distance from the centre and the edges of the terminal face, is of a blue

colour, while all the rest of this face is gray, slightly tinged with red, and *chatoyant*. (Fig. 36.) In the other, the last concentric hexagon alone, or that which at the same time forms the exterior part of the crystal, is (for the thickness of about half a line) of a blackish-brown colour, dull and opaque; while the rest of the terminal face (which likewise exhibits concentric hexagons) is of a gray colour, but has a silvery hue, because this part of the stone is *chatoyant*. (Fig. 37.) The above circumstance seems to announce a deposition of laminæ upon the sides of the hexaedral prism; nevertheless, if we attempt to break these crystals according to that direction, we find that it is absolutely impossible to succeed, in such a way as to obtain a fracture that has the appearance of being made in the natural joints of the stone; whereas, on the contrary, fractures may be made with sufficient ease, in a direction corresponding to the faces of the primitive rhomboid. Notwithstanding these concentric hexagons, there may be sometimes perceived, upon the same terminal faces of the prism, traces of the edges of the laminæ already mentioned; and the crystal then exhibits the appearance represented in Fig. 38. As the real direction of the laminæ (which is shown in these crystals by their natural fractures) indicates that the rhomboid of 96° and 84° is the primitive form of this substance, it seems necessarily to exclude the other direction, of the existence of which (as we have seen) there is some appearance, and which would give the hexaedral prism, as the form of the primitive crystal.

The above appearance, however, is certainly owing to a particular cause; but it seems to me, that the laws hitherto established in crystallography, are by no means capable of furnishing one that can account for it in a satisfactory manner. The only

explanation of the circumstance which occurs to me, does not agree with the idea we have formed respecting those laws; but the circumstance itself may be perfectly explained by it. It is founded upon a supposition that the primitive rhomboid may have passed, very nearly at the time the crystallization began, to the form determined by the combination of the two modifications which produce the hexaedral prism, and that, in consequence of a law not yet acknowledged, the sides of the prism may have become, at the very moment of their formation, a new centre of attraction, for the regular deposition of a part of the crystalline molecules. This supposition, however, would require another, but which perhaps may be fairly considered as nothing more than a consequence of the former, namely, that the mutual attraction of the molecules situated upon these secondary faces, is more strong than that which exists in the same way between those upon the primitive ones. This stronger degree of attraction between the molecules on one of the faces of a crystal than between those of the other, is already admitted; so that it may rather be considered here, as giving rise to an additional observation, than as affording matter for discussion. I am perfectly sensible of, and make no scruple to allow, every objection that may be made against this explanation, to arrive at which, I have been obliged to make a supposition not yet admitted; but the fact itself exists, and seems naturally to lead to the explanation I have given. I offer it, however, merely as a hypothesis, which still requires the support of observation; and I shall only add, that it is not the first time that the study of crystals has led me to form such an idea.

With respect to the cause which, notwithstanding the above-mentioned mode of crystallization, would still occasion the frac-

ture to have the same direction as if the increase of the crystal had been produced by a deposition on the faces of the primitive rhomboid, it may, I think, be explained by supposing that, in this case, the elements of the crystallization might already be real, though small, secondary crystals, for instance, small hexaedral prisms; and that the fracture would then be nothing more than the result of the sum of all the partial fractures of each of them.*

PHENOMENA WITH RESPECT TO LIGHT.

The prismatic crystals of corundum, as well as the pyramidal ones, when their extremities are terminated by faces which are perpendicular to their axes, very frequently have those terminal faces *chatoyant*. This property is the natural effect of the

* I had finished writing this Paper, when Mr. Greville had the curiosity to cause one of the hexaedral prisms of imperfect corundum, from the coast of Malabar, the terminal faces of which exhibited the concentric hexagons above spoken of, to be cut transversely. This section shewed a very interesting fact, and one that adds some probability to what I have said respecting the cause which produces this phenomenon. One of the parts of this crystal (which crystal is sawed into three, and polished,) exhibits the appearance represented in Fig. 38, A. The whole substance of this segment is of a pale purplish-red colour; but there is, in its centre, a triangular spot, similar to that represented in the above figure, which indicates very clearly that the section was made below the summit of the primitive rhomboid, and perpendicularly to its axis. This spot is also of a purplish-red colour, but much more deep than the rest of the crystal, and therefore strikes the eye very forcibly. It is only to be perceived upon one of the terminal faces; the other terminal face does not shew the smallest trace of it. There may, however, be perceived at its centre, a hexagonal plane, nearly as large as that represented surrounding the spot in Fig. 38, A; it is of a different colour from the other part of the substance of this segment, being of a dirty gray. The spot is also seen, but of a smaller size, upon the terminal face corresponding to the segment taken from the top of the preceding; but there are not any traces of it upon the other terminal face.

reflection of light, in the small intervals which remain between the small crystalline laminæ, in those parts where these laminæ are not in perfect contact; it is necessary, therefore, that the crystal, or fragment, which possesses this property, should be in the state most favourable to its developement. On this account, it must not be completely transparent; there being, in that case, too perfect a contact between the laminæ; so that the light, not meeting with any medium to reflect it, but being entirely refracted, cannot occasion any appearance of the property here spoken of. Neither must the crystal, or fragment, be quite opaque; it being necessary that the light, in order to undergo the reflection which produces this pleasing phenomenon, should at least be able to pass through the exterior laminæ of that part of the crystal against which it strikes. The above circumstances are, in fact, those which appear to take place with respect to corundum. The imperfect corundum of the Carnatic, the crystals of which are generally more or less opaque, show no trace of this property upon their terminal faces; whereas, it is frequently observed upon the terminal faces of the crystals of imperfect corundum from China, and also of that from the coast of Malabar, because those crystals generally possess a slight degree of semi-transparency. This character is still more common in the perfect corundum, whether sapphire or oriental ruby. There is not, however, the smallest appearance of it, when these stones possess the beautiful transparency belonging to them in their highest degree of perfection; whereas, on the contrary, it is frequently seen to take place in a very lively and brilliant manner, in such of the stones as have an inferior degree of transparency. It rarely happens, that the crystals of perfect corundum are prevented by opacity from exhibit-

ing the property here treated of; but, as I have already said, the terminal faces which, by their position, replace the solid angles of the summit of the primitive rhomboid, are absolutely the only ones which can in any degree possess it: no appearance of it can be seen in any other part. This is not surprising; for, as the effect here spoken of proceeds from the reflection of light, in the spaces between the crystalline laminæ, the plane which may be considered as produced by a section which would expose the edges of all these laminæ, must necessarily be the most proper to occasion it. This effect also takes place when the crystals are broken, by chance, in a direction more or less approaching to that which is parallel to the abovementioned plane, notwithstanding the fracture then exhibits a very rugged appearance. It even happens sometimes, that this fracture is such that the edges of the laminæ protrude, in the manner observed in the fibres of wood when it is broken across the grain; yet the property here treated of is not less evident; and, in this last case, it is often very distinctly seen proceeding from between the laminæ.

To the above property must also be referred, that beautiful reflection of light, in the form of a star with six rays, which is frequently given, by cutting, to oriental rubies, sapphires, &c. and which causes those stones to be then called by the name of *star-stones*. The manner of cutting which brings the perfect corundum into this state is, most commonly, on the part of the lapidary, rather the result of chance, than the consequence of any determined theory respecting the cause of the effect he means to produce. Accordingly, in the greater number of the stones which have this property, the point from whence the starry reflection proceeds, instead of being in the middle

of the stone, is observed to be situated in a part more or less near to its base; a circumstance which considerably diminishes the beauty of the star-stone. The reflection which produces this effect, arises from the same cause as that of which we have already spoken, and proceeds from the same part of the stone; consequently, when an oriental ruby, or a sapphire, which has the qualities necessary for the purpose, is intended by the lapidary to be formed into a star-stone, he ought to make his section pass below that part of the stone which he has found to correspond with the summit of the primitive rhomboid. As the kind of cutting most proper to produce this effect in the stone, is that rounded form which is called *en cabochon*, with as high an ellipsis as is possible, the lapidary ought, at the same time, to take great care that the summit of this ellipsis be situated exactly under the point which corresponds with the summit of the rhomboid; in that case, the light reflected in the interval of the laminæ upon the three edges of the primitive rhomboid, and upon the middle of its three faces, will trace upon the stone, a star, the six rays of which will include the circumference of the rounded part, or ellipsis. The same effect may also be made to take place on one of the solid angles of the base, but in a much less perfect manner.

I have met with many fragments of sapphires, as well as of oriental rubies, which naturally produced the effect here spoken of, in consequence of their having been broken, by chance, in a manner proper to occasion it; that is, they were broken, accidentally, in a direction contrary to that of the laminæ, and perpendicular to an axis passing through the two summits of the pyramid of the primitive rhomboid; after which, the fragment had been a little rounded by friction.

The imperfect corundum may likewise be cut in such a manner as to produce the starry reflection; but it is more rare than in the perfect kind, to meet with pieces which have all the qualities requisite for this purpose. There is, in Mr. GREVILLE's collection, a large piece of imperfect corundum, of a brown colour, which has been cut *en cabochon*, with the above-mentioned intention; but, the cutting not having been made in the proper direction, the starry reflection is exhibited in a very imperfect manner, as it proceeds from a point near the plane of the base of the stone. The effect produced, however, is sufficient to remove all doubts respecting the existence of the property here spoken of, in this kind of corundum.

CHARACTER AFFORDED BY ANALYSIS.

In order to complete the proofs I have already given, that all the stones which form the subject of this Paper are of one and the same nature, I shall borrow this last mentioned character from the analyses made by Mr. CHENEVIX, which will hereafter be described at length by that able chemist; and it may be observed, that few instances can be met with where the chemist and the mineralogist, after having jointly employed themselves in their different provinces, upon the same substance, have arrived at a more satisfactory and correspondent result.

According to Mr. CHENEVIX's analyses, the constituent parts of the various substances here treated of, are as follows.

IMPERFECT CORUNDUM.

	From the Carnatic.	From Malabar.	From China.	From Ava.
Silica	5,0	7,0	5,25	6,5
Alumina	91,0	86,5	86,50	87,0
Iron	1,5	4,0	6,50	4,5
Loss	2,5	2,5	1,75	2,0
	100,0	100,0	100,00	100,0.

PERFECT CORUNDUM.

	Blue, or sapphire.	Red, or oriental ruby.
Silica	5,25	7,0
Alumina	92,0	90,0
Iron	1,0	1,2
Loss	1,75	1,8
	100,00	100,0.

From what has been said it appears, that the analogy existing between the stones hitherto known by the names of corundum, sapphire, oriental ruby, oriental hyacinth, &c. is so strong and complete, as no longer to permit us to doubt that they ought all to be considered merely as varieties of the same substance, to which I have therefore given the general name of corundum.

In the learned work on mineralogy which the Abbé HAUY has just published, this celebrated naturalist says, that nearly at the same time I communicated to the Royal Society my first observations on this substance, he had himself observed the existence of corundum, among the crystals of

different substances contained in the sand of Ceylon; having, as he says, seen therein some small hexaedral prisms, of a ruby red colour, and transparent, which, from the analogy that appeared to exist between their external characters and those peculiar to corundum, might very naturally be ranged with that substance. Some particular circumstances certainly prevented him from making the same observations respecting the pyramidal crystals, of the above colour, which are also found in that sand; and he consequently thought it right, (although he appears to have had some doubts upon the subject,) to continue to separate the sapphire from corundum, giving to the former the name of *telesie*: indeed he has placed them at a considerable distance from each other, the sapphire being the third species of his second class of stones, and the corundum the fourteenth. What he seems to consider as the strongest arguments in favour of this separation, are, the laminated texture so evident in all crystals of corundum, and the direction of the laminæ being according to the inclination of the faces of a rhomboid; whereas, in the sapphire, this laminated texture seemed to him not to exist; and he adds, that the fractures of sapphire appeared to him to follow a direction perpendicular to the long axis of the crystal.

With regard to this, I shall observe, that in the foregoing descriptions of the characters peculiar to this substance, (which have been given with all the circumstantial detail necessary in a demonstration which is intended to leave no doubt upon the subject,) the observations of the Abbé Hauy appear to me to have been completely answered. It has there been stated, that one of the peculiar properties of this stone was, that it always preserved a very distinct laminated texture, in all those varieties

wherein the crystallization appeared not to have attained its highest degree of perfection, which varieties I have distinguished by the name of imperfect corundum. But it has also been stated, that in proportion as the crystallization possessed a greater degree of perfection, the texture exhibited a less laminated appearance; and that, in this case, it was less easy to obtain a fracture in the real direction of the laminæ.

Another circumstance has likewise been taken notice of, which appears to me to deserve some attention, namely, that in all the different varieties of this substance, the blue colour was generally accompanied with a greater degree of transparency, of gravity, and of hardness; and that, under these circumstances, in proportion as the adhesion of the laminæ was more complete, the laminated texture of the stone became less evident, and it was much more difficult, and sometimes scarcely possible, to obtain fractures in the direction of the laminæ. Nevertheless, among crystals and fragments of sapphire which had but a small degree of transparency, I have frequently met with some, in which the laminated texture was as evident as in the red prismatic variety of perfect corundum, or oriental ruby.

With respect to what concerns the fracture of the sapphire, if the Abbé HAUY was not deceived by an illusive appearance by no means rare in this stone, both in its perfect and imperfect state, (according to which the terminal faces seem to indicate a laminated texture perpendicular to the axis, or a fracture in that direction,) I cannot account for his thinking that he had obtained such a fracture as he describes. I have often tried to obtain fractures of that kind, but without success; never having been able to procure any, except such as were more or less irregular, and exhibited an appearance very different from that of fractures

made in a natural direction. Moreover, I have examined a great number of crystals of sapphire, many of which had one of their extremities, many others both their extremities, broken in a direction approaching more or less to that which is perpendicular to their axes, but have never seen, among these fractures, any one that had the appearance of being made in the natural direction of the laminæ; although, at the same time, I have, in many crystals, seen fractures which were perfectly even, and often of considerable extent, in the direction of the planes of a rhomboid, exactly similar (with respect to the measure of its angles) to that belonging to the primitive crystal of imperfect corundum. I have already observed that there sometimes remain, upon the terminal faces of the crystals of sapphire, small facets belonging to the above planes.

I cannot help mentioning also, in this place, a very interesting crystal of sapphire, that is in Mr. GREVILLE's collection. This sapphire, which is of a pale blue colour, is a simple hexaedral pyramid, the solid angle of whose summit measures 40°, and retains upon one of the angles of the summit, which is incomplete, a large triangular facet, belonging to one of the planes of the primitive rhomboid. This plane is striated transversely, in a manner that shews some derangement in the crystallization, perhaps from too great rapidity; and, in the upper part, a still more rapid decrease changes its degree of inclination, causing it to take one which is greater, and which belongs to the secondary obtuse rhomboid already described, in speaking of the fifth modification. These planes, together, completely terminate the crystal at this extremity, in the manner represented in Fig. 39. There may also be observed, two other planes, between which is comprehended the plane I have just described as one

of those of the primitive rhomboid: they are produced by the passing of the crystal to a less obtuse pyramidal modification.

Corundum is not the first mineral substance that has exhibited, even in its crystallized state, very striking differences, according to the circumstances that have governed its formation, and the greater or less degree of perfection that has taken place in its crystallization. Felspar is a substance to which the very same remarks may be applied. In the interior part of most kinds of granite and porphyry, it appears in the form of very rugged crystals, generally opaque; whereas, in the fissures of primitive rocks, it frequently has a beautiful transparency; and, when this happens, it rather exceeds the former kind in hardness and in gravity. This difference, which for a long time prevented the latter kind from being joined with the felspar of granites, is so striking, that most naturalists have thought it right still to continue to separate it, at least as a variety, although they allow it a place in the same genus, under the name of *adularia*.

There exists also in the same genus (felspar) a third variety, which, though it had long been known by the name of *white schorl of Dauphiny*, was not, till lately, brought into its proper place. This kind of felspar, which is still more perfect, presents, in such of its crystals as have the greatest degree of transparency, a brilliancy that is even superior to that of the most perfect adularia; this transparency is less similar to that of glass, and approaches nearer to that which is peculiar to the stones that have been hitherto distinguished by the names of gems or precious stones. Indeed, it always appeared to me to possess, in general, the two characters of hardness and gravity, in a somewhat greater degree than adularia. It rather scratches

adularia than is scratched by it. In the division which I usually make of the different kinds of felspar, I distinguish this latter, in consequence of the above-mentioned character, by the name of *brilliant felspar*.

We shall see hereafter, that there probably exists a fourth variety of felspar, without reckoning that which is known by its greasy aspect. The fracture of this greasy kind is dull, and resembles that of wax. It exists, in great quantity, in certain granite rocks, which usually abound with hornblende; of which rocks there is a great number in Scotland. In these, it is frequently of a green colour, which gives it exactly the appearance of jade. This kind of felspar may very probably be a particular kind of substance, nearly allied to one of those (very different from each other) to which French mineralogists give at present the name of *petrosilex*.

COMPACT CORUNDUM.

We have hitherto seen corundum only in a form more or less perfect or determined; it is, however, sometimes met with in a state in which there does not appear the smallest rudiments of crystallization. In this state, (to express which, mineralogists have agreed to make use of the term *compact*,) it resembles, in many respects, a coarse jasper; but its much greater degree of hardness, and its much higher specific gravity, render its true nature easily distinguishable.

In Mr. GREVILLE's collection are many specimens of this compact corundum; they are all of a purplish red colour, not very deep, and are perfectly opaque. By means of a lens, there

may be perceived, here and there, some small particles, in which an incipient laminated texture is discernible. These particles are rendered visible by the reflection from the laminæ; they are of a beautiful rose colour, and have a slight degree of transparency. The lens also shows, at the same time, a great number of small globules, of a deep black colour, and of a very brilliant lustre: these globules do not consist of attractable oxide of iron, although that oxide is very common in the substance here treated of; but, on account of their small size, it has not yet been possible to determine their nature.

The compact corundum of a red colour gives pretty strong sparks, when struck with steel; it also gives, by collision, the same phosphorescent fiery red light as the other red varieties of corundum, both perfect and imperfect.

The mean specific gravity of compact corundum, taken from three trials, which differed very little from each other, was 3902.

MATRIX OF IMPERFECT CORUNDUM FROM THE PENINSULA OF INDIA, AND CHIEFLY FROM THE CARNATIC.

This matrix, which, as far as our present knowledge extends, appears to be peculiar to the imperfect corundum of this part of Asia, is a stone of a particular nature: it is sometimes in masses of a loose and granulated texture, with very coarse grains, and pretty much resembles a coarse sand stone; at other times, it has a closer texture, the grains being nearer each other, and less distinct, so as either to give it an appearance similar to the kind of marble known by the name of *coarse-grained saline marble*, or to that kind of prehnite which is composed of a mass of

crystals confusedly aggregated. In this matrix, the crystals of imperfect corundum are dispersed, in the same manner as those of felspar are dispersed in porphyry, or rather in certain granites which, besides the aggregated constituent parts belonging to that kind of rock, also contain crystals of felspar which are of a more or less considerable size, and of a perfectly determined form.

When this substance is of that texture in which the grains are closely connected together, it is of a pearly gray colour, sometimes slightly tinged with green, and has a degree of semi-transparency, not unlike that of calcedony. If a piece of this kind is moved about in a strong light, its surface shows a considerable number of small brilliant particles. This appearance arises from the reflection of the light, by the small laminæ that are exposed, in consequence of the fracture of the grains of which the stone consists; and this circumstance proves that it has a laminated texture.

In the last mentioned state, (the most perfect one in which I have observed this stone,) its hardness, although sufficient to scratch glass very easily, is rather inferior to that of felspar. It gives sparks when struck with steel; and, by means of strong collision, emits a phosphorescent light, of a bluish white colour. Friction does not produce any signs of electricity. When put into nitric acid, no effervescence was perceptible.

The specific gravity of this stone, as determined by four trials, which scarcely differed from each other, was 2·742; but it is difficult to procure pieces of a tolerable size, which are not mixed, either with hornblende, or with particles of corundum.

It is fusible by means of the blowpipe.

This substance is more usually met with in pieces of a coarser

texture, in which the grains are often pretty large, so as to be easily distinguishable by the naked eye. When these pieces are in a perfect state, the grains have exactly the same colour, and the same degree of semi-transparency, as those of the preceding more compact kind. If examined with a lens, the laminated texture of these grains is very evident; and there seems to be, at the first view, a very distinct crystal in each of them. But, if we endeavour to determine the form of any one of these crystals, we find that it is absolutely impossible to do so; as the greatest part of the small facets we perceive, are nothing more than facets formed by compression. I thought, indeed, that I could distinguish some traces that indicated an obtuse rhomboid; but not in such a manner as to permit me to state the fact with certainty. These grains have but a weak degree of adherence to each other; in consequence of which, the stone may often be broken by a very slight effort.

It is, however, still more common to meet with this substance in a state wherein it has undergone, at the surface of each of the grains of which it is composed, an incipient decomposition, that gives them a whiter colour, thereby obscuring, and indeed often destroying, that semi-transparency which I mentioned as being a character of this substance, in its two preceding states. When this is the case, if some pieces of the stone are put into nitric acid, an effervescence soon takes place, the strength of which is in proportion to the degree of decomposition the stone has undergone; but this effervescence, in a short time, entirely ceases. It seems, from this circumstance, that the lime contained in the stone, (which, as will be hereafter seen in the account of its analysis, Mr. CHENEVIX found to amount to 15 parts in 100,) being exposed to the action of the air, by the

alteration or decomposition of the stone, had afterwards combined with a portion of carbonic acid.

To the above mentioned lime, (carried away by the rains which wash the exposed parts of the rocks composed of this substance, and deposited upon the fragments of corundum scattered at the feet of those rocks,) ought no doubt to be attributed, that calcareous incrustation which is frequently observed to cover, either partially or entirely, many fragments of imperfect corundum, found among the specimens of that substance sent to us from India.

If we let a piece of this matrix remain for a certain time in nitric acid, it is attacked by it, without being dissolved, and without changing its form; but if, after being taken out, it is pressed between the fingers, it may be crumbled by a very trifling effort, and may, by being rubbed, be reduced to a sort of paste.

SUBSTANCES WHICH ACCOMPANY THE IMPERFECT CORUNDUM, IN THE ABOVE MENTIONED MATRIX, FROM THE PENINSULA OF INDIA.

Felspar. There are sometimes found, in the matrix here treated of, pieces, more or less considerable in size, of a laminated substance, which has the same greenish gray colour, the same brilliancy, and, in short, the same appearance, in many respects, as the corundum itself. It is indeed the more easy to confound this substance with corundum, as it is frequently accompanied with crystals of the latter. I have myself been several times led into this mistake, before I had paid such particular

attention as I have since done, not only to corundum, but also to every thing relating to the substances which accompany it.

The most usual colour of this substance, as I have already said, is gray, slightly inclining to green, which is sometimes mixed with a small portion of brown. It possesses a pretty considerable degree of semi-transparency, which may be compared to that of calcedony, or more properly to that of the stone known by the name of cat's eye. Its hardness is inferior to that of quartz; but appears to be exactly the same as that of felspar. It gives sparks, when struck by steel; and, by collision, emits a yellowish phosphorescent light. Friction does not cause it to give any signs of electricity.

This stone may be divided with great facility, in the direction of two opposite and parallel faces; and the fractures thereby obtained have a brilliant lustre, exactly resembling that of the fractures of corundum. Upon these fractures may be observed very fine but very evident striæ, which indicate that the laminæ have a direction different from the above; but I have not yet been able to obtain an even fracture, in the direction of these striæ. All fractures made in any other direction than that first mentioned, are irregular and unequal; very often also they are dull, and somewhat similar to that of wax.

The mean specific gravity of this substance, taken from four trials, which differed very little from each other, is 2643.

This substance is fusible by the blowpipe, like common felspar.

The result of the analysis of this substance, made by Mr. CHENEVIX, is, in many respects, similar to that of the analysis

of adularia, made by Mr. VAUQUELIN; yet it differs very essentially from that, by the want of potash, and by the proportion of lime being more considerable.* The presence of the last-mentioned earth is sometimes rendered evident, in the parts which are slightly decomposed, by the weak and momentary effervescence that takes place in those parts, when the substance is put into nitric acid.

On the other hand, many of its external characters are such as naturally lead to its being ranged with adularia. It differs from it, however, in the facility with which the latter may be broken in two different directions; while, in the substance here treated of, fractures can never be obtained, except in one of those directions; nor have I ever been able to observe on the fractures of any other kind of felspar, those fine striæ which,

* The analyses made by Mr. VAUQUELIN, of the different kinds of felspar, naturally lead me to make some further remarks upon that substance; which, indeed, may be equally applied to many other substances. The able chemist above mentioned, found 14 parts of potash in 100 of adularia, and 13 in 100 of the green felspar of Siberia; whereas, he did not find an atom of that substance in another kind of felspar, which was in a laminated mass; nor in that decomposed felspar which is known by the name of kaolin. Potash may therefore be considered as not being one of the constituent parts of felspar, but merely as a foreign substance, accidentally interposed therein. Adularia, in that case, would be nothing but an impure kind of felspar; and would present the astonishing phenomenon of a substance constantly impure, in its most perfect state of transparency and crystallization. It is indeed difficult to conceive that the potash is merely interposed, in such very considerable proportion, in the kind of felspar called adularia; yet, if it really formed one of its constituent parts, it would necessarily produce a substance totally different from those which do not contain any of it; whereas, all the mineralogical characters of felspar and adularia, evidently demonstrate that these two substances are perfectly similar in their nature. There still remain, in my opinion, many discoveries to be made, in that part of chemistry which relates to the composition of mineral substances, before the chemist and the mineralogist shall be enabled to proceed together, with a certainty of agreement respecting the object of their inquiries.

as I have already said, are very evident on this stone. It differs also from common felspar, in not being capable of acquiring electric properties by friction; whereas common felspar may, by long continued friction, be made to acquire such properties. The semi-transparency of this stone likewise, and the nature of its lustre, are such as give it a greater analogy to gems or precious stones; and, in these respects, it is very similar to the variety which I have called shining felspar.

As this substance appeared to me to have a great analogy with another, which sometimes, in small fragments, accompanies the perfect corundum in the sand of Ceylon, (in which, however, they are more rare than corundum itself,) I desired Mr. CHENEVIX to be so good as to add to the analyses he was about to make, that of these fragments. The result of his analysis of them differs so little from that afforded by the substance above described, that it strongly confirms the analogy I had supposed to exist between them.

Having been so fortunate as to find, among the few fragments I could collect of the last mentioned substance, three crystals, in which the crystalline form is perfectly determined, I am enabled, by their means, to add the crystalline character of the substance, to those I have given in the foregoing paragraph. These crystals are rhomboidal tetraedral prisms, of about 100° and 80°, the two terminal faces of which are inclined, in a contrary direction, upon the obtuse edges of 100°, in such a manner as to make with them, an angle of 105° on one side, and one of 75° on the other; and as, (in the only three crystals it has yet been in my power to examine,) the planes of the prisms are very nearly equal to the terminal faces, their appearance is exactly that of a rhomboid. The terminal faces of the crystals

here spoken of are *chatoyant;* and, in the fragments, the planes which correspond to these faces have a similar property, when held in a proper direction. In some, these faces then appear of a pearly white colour; in others, the colour is rather yellowish: some of them reflect a pale blue colour; in many others, the colour reflected is a beautiful deep sapphire blue, that entirely occupies the whole extent of the face which possesses the property here spoken of. To this stone ought to be referred, that which is known by the name of moon-stone of Ceylon, when it is not of the kind called *cimophane*, (the *chrysoberyl* of WERNER,) which is often found also in the sand of this island, mixed with rubies, sapphires, &c.

The opinion I am naturally led to adopt, in consequence of the detail I have just given respecting this stone, is, that it most probably is a kind of felspar, and ought to be ranged with that substance, as forming an additional variety.

In some of the pieces of this stone, which are found in the same matrix with the imperfect corundum of the Carnatic, a talcy earth (which often also appears in a separate state) is interspersed throughout their substance, and causes them to have a less compact texture, and a very inferior degree of hardness. The stone, at the same time, acquires a slight greasiness to the touch, and loses the semi-transparency which is peculiar to it: it may still, however, be easily divided, in the direction already described as that in which it is naturally divisible.

Fibrolite. The substance I have distinguished by this name, which sometimes also accompanies the imperfect corundum from the Carnatic, in its matrix, has always offered itself to my observation, either of a white colour, or of a dirty gray. Its hardness appeared to me to be rather superior to that of quartz; as, after

having rubbed them together, the latter seemed to be the most worn of the two. It gives bright sparks, upon being struck with steel. Collision causes it to emit a phosphorescent light, of a deep reddish colour. It cannot, by friction, be made to give signs of electricity.

Its mean specific gravity, taken from four trials, is 3214.

This substance was tried with a blowpipe, by Mr. FLEURIAU DE BELLEVUE, a mineralogist much accustomed to such operations, and found to be absolutely infusible, even when placed, in very minute particles, upon cyanite.

The external texture of this substance is usually fibrous; the fibres being very fine, and closely connected together. When it is broken according to the direction of the fibres, its internal texture appears to be exactly the same; but, if it is broken in a direction transverse to the fibres, its texture appears to be compact. The lustre of the last kind of fracture is rather vitreous; and there is nothing in its appearance that gives reason to think it was made in the direction of the laminæ. When we wish to try the hardness of this stone, it should be done in a direction which is transverse or perpendicular to the fibres; not in a direction parallel to them.

There exist many pieces of this substance that are merely irregular aggregations, in which the fibres cross each other, in bundles, in different directions. I have only once seen it in a form which could be considered as a determined one; viz. a rhomboidal tetraedral prism, of about 80° and 100°, the terminal faces of which are imperfect. But, as this prism, although pretty regular in its form, is the only one I have yet been able to discover, the above observation requires to be repeated, before we can safely make any dependence upon it. I must

however add, that among the pieces of this substance, I have met with several, which appeared to have more or less tendency to the above-mentioned form.

The analysis of this substance, made by Mr. CHENEVIX, concurs with the whole of its external characters, in warranting us to consider it as being different from any of the mineral substances hitherto known; in consequence of which, I have thought it right to distinguish it by the name of *fibrolite*.

Thallite. The substance called thallite (the *epidote* of the Abbé HAUY) also sometimes accompanies the corundum from the Carnatic, in its matrix. This substance is found in three distinct states, hitherto unobserved, in all of which its appearance is so different from its usual one, as to have prevented me, for some time, from knowing it.

In one of the above states, this substance is inclosed in the matrix, in small detached masses, from the size of a pea to that of a hazle nut, and even larger. Its usual colour is either a brownish green or a yellowish green; and it has only a slight degree of semi-transparency, even at the edges.

Its hardness is the same as that of the other known kinds of thallite, which I have always found to be rather superior to that of quartz; and, as most of the other characters belonging to this kind of thallite are similar to those of the kinds already known, I shall, in the following description, mention only such of its characters as, on account of their being different, might lead to false ideas respecting it.

The major part of these small masses present no determined form; in some of them, however, a perfectly regular crystallization may be observed. In this latter state, the greater number of crystals appear in the form of rhomboidal tetraedral prisms, of 128° 30′ and 51° 30′, in which the terminal faces are perpen-

dicular upon the sides, as in Fig. 40. (Plate IX.) This form, which was before unknown in the thallite, and which might at first view be taken for a primitive one, was very likely to lead to an erroneous idea; it may however be explained by another form, which is also met with in perfectly determined crystals. In these last, the prism is hexaedral, with two edges of 114° 30′, two others of 128° 30′, and the two last of 117°; its terminal faces are also perpendicular upon the sides of the prism, as in Fig. 41. Now this form is exactly the same as one of those already observed in the prism of the common thallite, and is produced in the following manner, viz. the primitive rhomboid, the edges of which are 114° 30′ and 65° 30′, has each of its acute edges replaced by a plane, inclined, in a contrary direction, upon one of the sides of the prism, so as to make with it an angle of 128° 30′. I have often found this hexaedral prism terminated, in the same way, by planes perpendicular to its sides, among the crystals of thallite from the Alps of Dauphiny. The preceding rhomboidal tetraedral prism, consequently, is produced by an increase of the faces which have replaced the edges of 65° 30′; which increase has been such as to cause the sides of the primitive rhomboidal prism, on which each of them incline, to disappear: this is represented by the dotted lines in Fig. 42. The direction of the laminæ, in these crystals, strongly supports the foregoing explanation. Sometimes the rhomboidal prisms become of an indeterminate form, by being flattened so as to render the edges of 128° 30′ much more obtuse; when that happens, they have no longer any regular measure.

In this first state of the thallite which accompanies the imperfect corundum from the Carnatic, the pieces, whether they are crystallized or of an indeterminate form, have their surface covered with little asperities, thereby exhibiting an appearance

which cannot be better described, than by comparing it to that preparation of fish-skin which is called shagreen. This is the natural effect of their peculiar texture; for, if one of these pieces is broken, we perceive very plainly, that it is not of a homogeneous texture, but is mixed with small particles of the substance we have already described as the matrix of corundum; which mixture is often in such proportion, that the quantity of the latter substance is equal, or nearly so, to that of the thallite itself.*

The appearance the surface of these pieces exhibits, is owing to the destruction, at the said surface, of the forementioned small particles of the matrix, which, as is well known, is very easily decomposed. There sometimes even remains, in the little cavities, which are very numerous, small particles of this matrix, generally in a state of decomposition. In this case, if the pieces are immersed in nitric acid, a slight and momentary effervescence takes place; and, if this immersion is continued for some days, the acid then acts upon those particles of the matrix which are inclosed in the interior part of the substance, as has been already mentioned in the description of this matrix;

* The regularity of the form in which these crystals are found, will certainly appear surprising, when we consider the immense quantity of heterogeneous particles which are interposed within their substance, and, consequently, between their crystalline molecules, the attraction of which for each other, it would appear, must be thereby considerably obstructed; but the same circumstance takes place in other substances, for instance, in the calcareous spar known by the name of *rhomboidal sand-stone of Fontainbleau*. The Abbé HAUY, in the article *axinite*, (the *thumerstein* of WERNER,) makes the same observation, and gives a very ingenious explanation of the circumstance. This calls to our mind the remark of the celebrated DOLOMIEU, viz. that it appears, in some cases, that a foreign substance, when interposed in a crystal, instead of obstructing its crystallization, tends rather to give it a greater degree of regularity.

in consequence of which, the pieces, when taken out of the acid, may be easily crumbled by the slightest pressure of the fingers; and nothing remains in its former state, except the small particles of the thallite.

There exist some pieces, in which the particles of the matrix are infinitely more numerous than those of the thallite itself; the latter then only appears in the form of small greenish or yellowish points, disseminated in greater or less proportion, and in detached spots.

In the second of the states in which this substance is found in the matrix of corundum, it appears in the form of pretty thick prisms; these prisms have deep grooves or channels, which, as is often observed in the crystals of tourmalin, render their shape absolutely deformed. The substance, in this second state, is more pure; no particles of the matrix, which were said to be mixed with it in its first described state, are to be seen. The semi-transparency is more general, and in a greater degree. The green or yellowish colour is also more deep; and sometimes a slight tinge of red is mixed with those colours. Some parts of the pieces are less grooved than others; and those parts indicate the forementioned rhomboidal prismatic form of 128° 30′ and 51° 30′; but it is very difficult to obtain an even fracture of this stone.

In the third state, this substance is so very similar to the purest imperfect corundum, that at first I supposed it to be of the same nature; and it was not until I had examined it more particularly, that its specific gravity and its hardness, so different from those of corundum, led me to think it could not possibly belong to that substance, and that it ought, from those characters, to be ranged with the thallite. The analysis of

it, made by Mr. CHENEVIX, has proved the truth of my observations.

Its semi-transparency, in this state, is more considerable, and approaches very nearly to complete transparency. Its colour is generally a beautiful topaz yellow, which sometimes inclines slightly to green. I have hitherto met with it only in pieces of an indeterminate and irregular form, the size of which, though more or less considerable, never exceeded that of a small nut. Its fracture is generally irregular, and often partially conchoid. In some pieces, however, may be perceived small particles which seem to have a laminated texture, the direction of the laminæ being such as to announce the primitive crystal of the thallite; but I have never been able to bring this substance to the shape of that crystal, by any artificial division or fracture of it.

Hornblende. This substance is that which is most constantly, and most abundantly, contained in the matrix now treated of. There are indeed some pieces of the matrix, wherein the proportion of hornblende is as great as in some granite rocks of which it constitutes the principal component part; and those pieces have an appearance very similar to that of such rocks. It is generally of a deep black colour, and opaque; but I have sometimes seen it in the form of small elongated crystals, of a fine green colour, and transparent. Its texture is very evidently laminated; and it is seldom that any determinate form can be perceived in it; sometimes, however, the rhomboidal tetraedral form of its prism may be distinguished.

Quartz. In this matrix is also found quartz, in small detached fragments, of an indeterminate shape. This substance, however, is by no means common; on the contrary, of the various

substances that are met with in this matrix, quartz is one of the most rare. It is generally of a dull white colour, and has but a small degree of transparency.

Mica and Talc. These two substances are not very common in this matrix, yet they are more so than quartz. The mica has a silvery hue, sometimes slightly inclining to green; and, in the pieces of the matrix in which it is found, it generally appears in small detached spangles.

The talc is generally of a pale green colour; and, in those parts of the matrix where it is met with, it is in pieces nearer each other than was the case with respect to the spangles of mica. Sometimes it forms small masses, little or not at all mixed with any other substance. At other times, it is found in that very divided or earthy state (seldom without some heterogeneous mixture) which has been hitherto distinguished, after Mr. WERNER, by the name of *chlorite*.

There are also, but more rarely, met with in this matrix, pieces of real steatite, of a white or a greenish colour.

According to a letter written from Tritchinopoly, the 10th of November, 1792, to Sir CHARLES OAKLEY, then governor of Madras, and communicated by him to Mr. GREVILLE, it appears that the imperfect corundum of the Carnatic, as well as the matrix in which it is contained, forms, in the place from whence it is procured, distinct strata; and that these strata are accompanied by a substance which is in considerable abundance, and which cannot be better distinguished than by the name of talcy mica. This substance is easily separated from the matrix of corundum; and it is usual to separate it, on the spot, before the pieces containing the corundum are sent away for the purposes of commerce. Some of it was sent to Mr. GREVILLE

by Sir CHARLES OAKLEY himself. The colour of this is a blackish brown; and its exterior appearance is nearly similar to that of mica; but the lustre of its surface is somewhat less bright. Its texture is very distinctly laminated; the laminæ, which are very thin, being chiefly evident at the edges; they adhere, however, more strongly to each other than those of mica. These laminæ may be bent, without breaking; but they do not show the smallest signs of elasticity. This substance possesses but a small degree of transparency, and that only when it is brought into the state of very thin laminæ; its colour then appears a brownish yellow, not much unlike that of resin. It is much more greasy to the touch than mica; it is also less hard, so that it may be easily scratched with the nail; and, if we scratch it with the point of a penknife, we are not sensible of that kind of slight shivering which takes place when mica is so treated. Mr. GREVILLE, in the Paper upon corundum which he presented to the Royal Society, in June, 1798, was perfectly aware of the difference between this substance and that properly called mica. In the collection he received of the former, are many crystals, several of which are nearly an inch in length, and two or three lines in thickness. Some of these are in the form of a rhomboidal prism, of 60° and 120°; others have the form of a regular hexaedral prism. Upon the whole, the characters of this substance may be considered as partaking both of those belonging to mica and those belonging to talc.

Its mean specific gravity, taken from three trials, which differed very little from each other, is 2709.

Garnets. In the matrix here spoken of, and also in the corundum itself, garnets are sometimes met with; they are of a deep

red colour, and of a roundish form. There was lately sent to Mr. GREVILLE, a parcel of imperfect corundum, found among the sands of the river Kirtna, in the district of Ellore,* in the northern part of the government of Madras. This corundum, some of the crystals of which were the best defined of any I had yet seen, was mixed with pretty large angular fragments of garnets, of a very deep blood-red colour, and of the most beautiful transparency.

Zircon. The same parcel of imperfect corundum, of which I have just spoken, from the district of Ellore, was also mixed with crystals of zircon, the jargon of the lapidaries. These crystals, which were in perfect condition, deserve to be mentioned, not only on account of their size, but also on account of the great number of varieties and rare forms they exhibit. Such, for instance, is the primitive very obtuse octaedron, which is in large crystals, with sides of more than six lines in length. I had observed this form, for the first time, fifteen years ago, in some crystals found in the sands of a rivulet, called Riou Pezzouliou, which runs between the volcanic rocks at Expailly, near Puy in Velay; but these crystals were very small. The celebrated ROMÉ' DE LISLE, who published my account of these crystals, in his excellent work on the external characters of minerals, mentions the opinion I then entertained, and had communicated to him, that the jargon and the hyacinth were only two differently-coloured varieties of the same substance, and were both derived from the same primitive form.

The most usual colour of these crystals of zircon, is a brown, which sometimes inclines to yellow: they often, however, have that fine yellowish red colour, which causes this stone to be

* This district is contiguous to that in which the diamond mines are situated.

distinguished by the name of hyacinth. Their size, and the perfection of their crystallization, enabled me to ascertain, that the angle formed by the meeting of the planes of the octaedron at the base, measures 85°; and that formed by their meeting at the summit, 95°; as is stated in the work I have just mentioned. The Abbé Hauy, in his excellent work on Mineralogy, fixes the first of these measures at 82° 50′, and the other at 97° 10′. I imagine he must have been deceived, either by the crystals having been of too small a size, or by their not having been of a perfectly regular form.

Amongst the pieces of the stone which serves as a matrix for the imperfect corundum, are found some, in which may be perceived a great number of very brilliant small points, of a yellowish red or orange colour. When viewed with a lens, these points appear to be minute crystals, perfectly transparent; but it is impossible to ascertain their form. On some of them may be perceived small facets; others have the appearance of prisms: they are of very considerable hardness. I am unable to form a decided opinion respecting the true nature of these microscopic crystals: but, all things considered, I am inclined to think it probable that they belong to the zircon.

Although these crystals, in the state I have just described, are extremely small, that state is by no means the smallest in which they are found in this substance; they also exist in it, so very minute in size, that our eyes, even when assisted with instruments, are scarcely able to distinguish them. In this state, they become a real colouring matter, for those parts of the matrix in which they are contained; which parts thereby acquire a fine orange colour, more or less deep. By attentively examining these parts with a lens of sufficient power, we may perceive

the crystals approaching nearer to each other, and diminishing in size, so as at last to become invisible: very often, they shew themselves only in the form of small filaments, scarcely perceptible.

The above is not the only substance which presents the phenomena just described, even in the stones here treated of; the thallite sometimes has the same appearances; and, in that case, it gives to the matrix a green colour, similar to its own. When this happens, we may sometimes, by means of a lens, perceive small microscopic crystals of thallite; very often, however, they are too small to be distinguished.

It appears therefore that coloured stony substances, by interposing themselves, in particles too small to be seen, in stones, may sometimes produce the same effects (and probably in the same manner) as are produced by the various metallic oxides.

Very attractable black Oxide of Iron. This ore of iron (which is the *fer oxidulé* of the Abbé HAUY, and the *magnetic iron ore* of the Germans,) is also found sometimes in the matrix of imperfect corundum from the peninsula of India; but, as we shall hereafter see, it is by no means so general, nor so abundant, in that matrix, as it is in the matrix of imperfect corundum from China. In the former, it appears in small grains of an indeterminate shape, which are sometimes interposed between the particles of hornblende, in such a way as might easily lead us to suppose, that the latter substance has the property of being acted upon by the magnet. In those parts of the matrix which contain this oxide of iron, are found hexaedral prisms of corundum, the surface of which is entirely covered by a layer of the oxide, about a quarter of an inch in thickness, and absolutely moulded upon them.

MATRIX OF IMPERFECT CORUNDUM FROM CHINA, AND SUBSTANCES WITH WHICH IT IS ACCOMPANIED.

This matrix is totally different from that of the imperfect corundum of the Carnatic, being a granite rock, composed of an aggregated mixture of felspar, fibrolite, mica, and very attractable black oxide of iron. I have not yet seen in it any particles of that particular substance, already described, which composes the principal part of the matrix of imperfect corundum from the Carnatic.

The four substances above-mentioned, are unequally distributed throughout the mass; some pieces being composed almost entirely of one of them; while, in other pieces, those substances are mixed together in various proportions, and sometimes in nearly equal ones. The crystals of corundum are disseminated in the mass, in the same manner as those of the Carnatic are in their matrix; but, as the particles of the matrix now treated of have a much stronger adherence to each other, and also to the crystals of corundum, it is difficult to detach the said crystals from the matrix, without breaking them.

The felspar has, in this matrix, the same appearance it usually has in granites. Its colour is generally reddish; very often, however, it is of a grayish white colour. I have never observed it to have any determined crystalline form; but, when it is in masses of a certain size, their texture is evidently laminated.

The mica has a silvery appearance, sometimes inclining a little to a yellowish colour, at other times to a greenish one. Its laminæ are frequently united together, so as to form prisms, which are pretty thick, but most commonly of an irregular

shape; sometimes, however, the appearance of a regular form may be observed in them.

The fibrolite is in much greater proportion in this matrix, than in that of the imperfect corundum from the Carnatic; and it is more generally dispersed throughout its substance; its fibres, however, are shorter, and form small detached diverging pencils, which unite together, crossing and penetrating each other in all directions, so as to present masses of a more considerable size. In this manner, it often entirely surrounds the crystals of corundum, and it is then impossible to disengage them from it. Its most usual colour is a whitish gray, but it is also frequently of a dull white. It is sometimes mixed, nearly in equal proportions, with felspar, and the attractable black oxide of iron; and thus produces a stone which, if polished, would have a very beautiful appearance. The analysis which Mr. CHENEVIX has made of this substance, concurs with all its other characters to demonstrate, that it is decidedly of the same nature as the fibrolite of which I have already spoken, as being found in the matrix of imperfect corundum from the Carnatic.

The very attractable black oxide of iron is, of the various substances found in the matrix of imperfect corundum from China, that which is most constantly, and most universally, mixed with it. In the smallest piece of this matrix that can be broken off, some particles of the oxide may generally be perceived; even the crystals of the corundum itself are hardly ever free from it, it being observable, not only upon their exterior surface, but also within their substance. This oxide of iron is usually disseminated, in this matrix, in small masses

of an indeterminate shape, which very often are nearly contiguous to each other. It is very rare to find among them any crystals perfectly formed; yet I have sometimes observed octaedrons, dodecaedrons, and segments of the first of these two forms, or octaedrons, which had in each pyramid, and exactly opposite, one of the faces much larger than the three others. This last form, appeared to me to be the most common one.

This oxide sometimes exists also in masses of a much larger size; but they are almost always of an irregular shape. I have often observed pieces as large as a hazel nut; and sometimes, though much less frequently, of a still more considerable size.

The mean specific gravity of this oxide of iron, taken from four trials, was 5073. This is rather superior to what has been considered as the specific gravity of this ore of iron, it having been always estimated at less than 5000. I know nothing to which I can attribute this difference, except to the peculiar texture of the oxide here described; which, as far as I have been able to observe, has always appeared to me to be much more compact than is usual in this species of iron ore. In other respects, it has, when perfectly pure, all the other characters belonging to this species.

There are some pieces of the matrix now treated of, in which the small masses of the above oxide, by being mixed with fibrolite and mica, exhibit an appearance that might cause them to be considered as pieces of a true granite; in others, it is mixed, in different proportions, with the substance of the corundum itself, in such a manner, that it is impossible, by the eye, to distinguish this mixture from the pure metallic oxide. Mr. CHENEVIX analyzed one of these pieces; and found that

it contained nearly equal quantities of corundum and of oxide of iron.

If, to what has been already said, I add, that there are sometimes found in this matrix, small pieces of green pulverulent talc, (chlorite,) and small masses of thallite, in thin elongated crystals, of a beautiful yellowish green colour, in the form of diverging rays, I shall have mentioned all the substances I have been able to observe, in the matrix of imperfect corundum from China.

Of the matrix of imperfect corundum from the kingdom of Ava, a small quantity only was sent; but that quantity was sufficient to demonstrate, that its nature is exactly the same as that of the matrix of imperfect corundum from China.

MATRIX OF PERFECT CORUNDUM FROM THE ISLAND OF CEYLON, AND SUBSTANCES WITH WHICH IT IS ACCOMPANIED.

I cannot help regretting, that it is not in my power to give much information respecting the matrix of perfect corundum from Ceylon. The precious stones comprised under that denomination, which are selected from the sands washed down by the rivers of the island, and sold under the name of *sand of Ceylon*, have never been brought to Europe in any kind of matrix, nor has any account of their matrix ever been transmitted to us. Perhaps, indeed, no more information on this head could be procured on the spot, than was obtained by those naturalists who sought for the origin of the sapphires, &c. found in the sands of the small rivulet at Expailly, already spoken of. I may also observe, that the great care taken to free the sand of Ceylon from every substance, except such as, on account of their

hardness and their lustre, are considered as of value in commerce, deprives us of all chance of obtaining that knowledge respecting the matrix here treated of, which might otherwise be acquired, from an attentive examination of the various substances which it is natural to suppose are brought down, with the sand, by the streams. We shall, however, presently see, that one of those fortunate events by which nature sometimes rewards the labours of those who devote themselves to the study of her works, has presented us with some very interesting facts on this subject.

In order to render as complete as possible, every information which is connected with the investigation of corundum in general, and particularly to make known every thing I have been able to learn respecting this stone in its highest degree of perfection, I think it right to make some remarks on the various substances with which it is accompanied, in the sand sent to us from Ceylon; although I cannot undertake to assert positively, that these substances really accompany it, when in its matrix.

Spinelle. The first of these substances, and one which composes more than nine parts in ten of the whole mass of the sand, is the spinelle ruby, now generally known by the name of spinelle. Notwithstanding the great number of crystals of this substance which are found in the sand, it is very uncommon to meet with one of a tolerable size, that is both transparent and of a perfect form: indeed most of them are merely fragments. The selection that has already been made in India, where these stones receive their first polish, in order to be distributed for sale, is no doubt the chief reason of the above circumstance:

we cannot therefore hope to find in the sand, any crystals of consequence, except such as have by accident escaped this first search; some of these, however, I have had the good fortune to meet with.

Among the beautiful series of crystals of this substance which I have been so happy as to procure, and to place in the several collections with the care of which I am entrusted by the friendship of their proprietors, there are four, in Mr. GREVILLE'S collection, that I think it right here to take notice of. The forms of these crystals appear to me to be hitherto absolutely unknown; for the Abbé HAUY, who may be justly considered as the most learned of those who devote themselves to the study of crystallography, does not even mention them, in the treatise on mineralogy he has just published.

One of these forms, is a complete tetraedron, as in Fig. 43. It is produced by the enlargement of four of the faces of the octaedron, at the expence of the other four, which it has entirely caused to disappear. There are, in the same collection, many other crystals which are passing into this form, and are more or less advanced towards it. One of them, in which there still remain some traces of the octaedron, which had entirely disappeared in the preceding, deserves also to be mentioned. This variety, which is more common than the preceding, is represented in Fig. 44.

The second of the above forms, is a very acute rhomboid, the rhombic planes of which have 120° for the measure of their obtuse angles, and 60° for the measure of their acute ones. Fig. 45. This crystal is produced by the enlargement of six of the faces of the octaedron, at the expence of two opposite faces,

one in each pyramid; which last faces have entirely disappeared. There are also several crystals in a progressive state, and more or less advanced, from the octaedron to this form. (Fig. 46.)

The third form, is a complete dodecaedron, with rhombic planes. Fig. 47. It is produced by the enlargement of the planes which have replaced the twelve edges of the octaedron; a modification to which the Abbé HAUY has given the name of *emarginée*. This enlargement is such as to have caused the entire disappearance of the eight primitive planes of the octaedron. There are also, in Mr. GREVILLE's collection, crystals more or less advanced towards this form, some of which no longer show any traces of the planes of the octaedron, except by extremely small equilateral triangular planes, as in Fig. 48. In these crystals, it is very common to find the decrease of the laminæ evidently indicated by striæ.

The fourth form, is a rectangular tetraedral prism, terminated by two pyramids, also tetraedral, which are situated upon the sides of the prism, and have equilateral triangular planes. This crystal is produced merely by the edges of the base of the octaedron being replaced; which replacement separates the two pyramids, by a prism more or less elongated. There are some crystals in which this prism is pretty long, as in Fig. 49; others in which it is, on the contrary, very short, as in Fig. 50.

Although the Abbé HAUY has described the cuneiform octaedron, I think it right to add to his description, that, in this variety, the separation of the two opposite faces in each of the pyramids, becomes sometimes so considerable, that the crystal thereby changes its appearance, and acquires that of a rhomboidal tetraedral prism, of 109° 30′, and 70° 30′. This prism is terminated by two diedral summits, with isosceles triangular

planes, the apices of which are situated upon those edges of the prism which measure 70° 30′, making with them an angle of 125° 15′, and meeting, by their bases, at the top of the crystal, in an angle of 109° 30′, as in Fig. 51.*

I also think it right to add, to what the Abbé HAUY has said respecting the colours of this substance, that it is sometimes perfectly colourless, sometimes of a yellow colour, and sometimes of a bluish one.

We were as completely ignorant of the nature of the stone which serves as a matrix to the spinelle, as we were respecting that of the matrix of the perfect corundum of Ceylon, when a number of specimens were sent from India to Sir JOHN ST. AUBYN, by Mr. WHITE, amongst which were two pieces of the highest value, inasmuch as they served to show us, for the first time, the substance now treated of, inclosed in its matrix. I flatter myself a description of these two pieces will be thought worthy the attention of the Royal Society, particularly as they also contain a species of iron ore hitherto unknown.

One of these pieces is a calcareous spar, of a granulated texture; the grains are very large, and are intermixed with each other, so as to adhere very strongly together, but their

* The dodecaedron, and the octaedron passing very rapidly to the tetraedron, had already been mentioned by Mr. ESLINGER, (*Journal de Physique*, Vol. LII. p. 225,) as making part of the collection of crystals of this substance in Mr. WERNER'S possession: the other varieties had not yet been described. According to some of the external characters by which Mr. ESLINGER describes the spinelle, I am inclined to think, that he includes some Ceylanites in that description, and also some oriental rubies. Such, for instance, I suspect to be, that which he says has a starry reflection; also the hexagonal prism with the alternate angles of the base replaced; and the cube, (without doubt, slightly rhomboidal,) which has a small plane upon two of its solid angles diagonally opposite to each other: a form that is very rarely met with, even in the oriental ruby.

fracture shows that they are very evidently laminated. In the substance of this spar are contained a great number of small prismatic crystals of mica, of a beautiful yellow colour, like that of the topaz; they have also the lustre, and the transparency, of that precious stone, for which they might the more easily be mistaken, as several of them, which show the sides of their prisms on the exterior part of the stone, appear to have their surface slightly rounded.* Very thin laminæ may without difficulty be detached from the terminal faces of the crystals; these laminæ are perfectly elastic.

There are also, in this calcareous spar, small pieces of a metallic substance, which deserves to be particularly described.

The colour of this substance is gray, slightly inclining to red, so as very much to resemble that of arsenical cobalt, or of nickel. The substance is very brittle; the slightest blow breaks it; and it may, by a moderate degree of pressure, be reduced into a black powder. Its fracture is conchoid, with a very fine and compact grain; and it has a very brilliant lustre. The magnet

* All the authors who have treated of mica, say that it is transparent only when in very thin laminæ. This is a mistake. When the crystals of this substance are in as perfect a state as they possibly can be, that is to say, when their crystalline laminæ are in complete contact with each other throughout the whole extent of their surface, (a circumstance very uncommon, but which is known by the sides of their prisms being perfectly smooth,) they are usually transparent. I have seen crystals of mica, of a pretty considerable thickness, which were perfectly transparent, in whatever direction they were viewed; although sometimes such crystals, when their terminal faces have a very shining silvery lustre, (which shows that they reflect all the light that falls upon them,) have not the smallest transparency, when viewed in a direction perpendicular to their axis; many of them, however, appear transparent, when viewed through the edges of the laminæ, that is to say, in a direction parallel to that of their axis. The above is not the only mistake that has been made with respect to this substance; a correct description of which, I hope, some time hence, to be able to lay before the Royal Society.

acts upon it, very nearly as strongly as it does upon iron in a perfectly metallic state. When this substance is immersed in nitric acid, no effervescence takes place. By means of a file, or merely by the blade of a knife, a black powder may easily be obtained from it, without in the least diminishing the lustre of the part from which it is taken. If a magnet be brought near this powder, it is instantly attracted by it. Those parts of this substance which appear to have been exposed for any length of time to the contact of the air, are become of a black colour.

I know no other metallic ore whose exterior characters are analogous to those I have just described; and I very much regret that the scarcity and the consequent value of this specimen, as well as of that about to be described, prevent their being made use of for the purpose of an analysis, the result of which it would be so desirable to be acquainted with. If, without such analysis, I might be permitted to form an opinion respecting this substance, I should be much inclined to consider it as a martial pyrites, or sulphuret of iron; but in which the iron, in a metallic state, is combined with a much smaller quantity of sulphur than in common pyrites; some small traces of the latter, however, may be perceived in this specimen, by the side of the metallic substance above described.*

In this same calcareous spar may also be observed, small crystals of a greenish colour, which have hexaedral prisms; they are of very inconsiderable hardness. I believe they belong to that particular species of phosphate of lime, which the Germans have distinguished by the name of *spargelstein*.

* Since the above was written, I gave a few grains of this substance to Mr. CHENEVIX; who, from that small quantity, was able to determine that it contained nothing but iron and sulphur.

But, what renders the specimen I am now describing, in the highest degree interesting, is, that there are some perfectly well formed octaedral crystals of spinelle, of a pale purplish red colour, inclosed therein. Here then we have a fair and unquestionable instance of the spinelle within its matrix: we shall however see presently, that the nature of this matrix is not constantly the same.

The second of the two pieces I have mentioned above, as being the matrix of the spinelle, is a mass of adularia, of a grayish white colour, about six inches in length, and of a proportionate thickness. This adularia is tolerably pure, in one half of the piece; but, in the other half, it is mixed with particles (much more considerable in size, and in much greater proportion than in the preceding piece,) of the very brittle and very attractable metallic substance already described. There may also be observed in it, some small pieces of a substance of a brownish green colour, but which becomes grayish when scraped; this substance, which is by no means hard, appears to me to be of the nature of steatite. If this specimen is moved about in a very strong light, there may be perceived in it, here and there, small particles, which have a silvery appearance, and which are rendered very evident, by their laminæ being in a direction contrary to those of the adularia which is near them. I consider these small pieces as belonging to the kind of felspar I have already described, and mentioned as being found in the sand of Ceylon which contained the perfect corundum and the spinelle, and as frequently reflecting a beautiful deep sapphire blue colour. This specimen contains fewer crystals of spinelle than the preceding one; some, however, may be perceived in it. It seems also to contain particles of calcareous earth, which

appear to be situated between the laminæ of felspar; at least, if a piece of it be broken off, and put into nitric acid, a slight effervescence is produced, which however is but momentary. These particles are most numerous, at those parts where the felspar and the metallic substance already described come into contact with each other.

I have placed a specimen of each of these stones in Mr. GREVILLE'S collection.

Notwithstanding there is a considerable difference in the nature of the matter which may be considered as the basis of these two pieces, yet the particular nature of the substances contained in them, which are perfectly similar to each other, seems to render it highly probable that the place of their origin was the same. But it also appears probable, from every circumstance respecting these stones, that they must have come, not from a mass of rock of the same nature as themselves, but from some veins, to the destruction of which may also very likely be owing the great quantity of spinelles contained in the sands of certain rivers of Ceylon. Would it be hazarding too much, to suppose that the crystals of perfect corundum which are found in this sand have also the same origin; and that (being much more rarely met with, and in much less quantity,) they have only a partial existence, or one that is confined to certain parts of the veins already spoken of. The small portions of felspar, and also of calcareous spar, which are sometimes, although very rarely, found in this sand, (perhaps because the sand has been already freed from such substances,) tends to support the supposition I have just made, namely, that these two substances are among those which compose the real matrix of the stones here treated of.

Tourmalin. This substance is also frequently found in the sand of Ceylon: indeed it is in this sand that the most perfect crystals of tourmalin, the most transparent, and the most various in colour, are generally found. It is certainly to be lamented, that these crystals are seldom of any considerable size; but that defect is compensated by the perfection and regularity of their form. Among these, I have found two in particular, of which, as they have not hitherto been noticed, I think it right to give a description.

The first of these forms, is the very obtuse rhomboid which is represented in Fig. 52, and is the primitive crystal of this substance. The Abbé HAUY, who also thinks that this rhomboid is really the primitive form of this substance, appears not yet to have met with it; for he has not placed it at the head of the description of tourmalin given in his mineralogy, as he has done with respect to the other substances of which he has observed the primitive form. It is indeed very scarce. I have, however, met with it several times; and have placed a very fine specimen of it in Sir JOHN ST. AUBYN'S collection. This crystal, which is about four lines in diameter, and nearly two lines in thickness, is of a brown colour with a tinge of orange; it is also pretty transparent, even in the direction of its axis. Its form is perfectly well defined; and the two pyramids, of which its rhomboid may be considered to be formed, are exactly similar to each other; neither of them having any supernumerary facets.

I think it right here to observe, that there appears to me to have been an error committed, with regard to the measures that have been given as those belonging to the primitive crystal of the tourmalin. The Abbé HAUY fixes the measure of the solid

angle of the summit of the pyramid at 136° 54′ 41″. ROMÉ DE LISLE's measure is nearly the same, namely, 137°. I have measured this angle with more than usual care, (on account of my not agreeing with these two celebrated naturalists,) having taken the precaution of using several different goniometers, and I have constantly found it to be 139°; which would make the angles of the rhombic planes 114° 12′, and 65° 48′, instead of 113° 34′ 41″, and 66° 25′ 19″, as stated by the Abbé HAUY.

The second of the forms abovementioned is a prism, either hexaedral, enneaedral, or dodecaedral, of which the terminal faces are perpendicular to the axis. This variety is produced in the following manner, viz. the plane that has replaced the solid angle of the summit of the pyramid, (which plane is represented by the Abbé HAUY in Figs. 119 and 120, Plate LII. of his Mineralogy,) has acquired an increase of sufficient extent to cause the planes of the pyramid entirely to disappear.

I think it right to add here, a variety of this substance, which also comes from Ceylon, and has not yet been described, namely, a prism which has become of a triedral form, with equilateral bases, by the enlargement of the planes that have replaced the three alternate edges; the formation of which planes is known to change the hexaedral prism into an enneaedral one; and the enlargement is such as to cause the six others entirely to disappear. The tourmalins of Ceylon are not the only ones in which I have observed this triedral prism: I have also met with it among the tourmalins of Saxony, and among those of Bohemia.

Lastly, I shall add, as forms not yet described, (although they do not belong to tourmalins of Ceylon,) two complete triedral pyramids, which, if they were not separated by an intermediate prism, would produce two secondary rhomboids, the

one more acute, the other more obtuse, than the primitive rhomboid.

The first of these pyramids, is the natural produce of the increase of the planes which have replaced the acute angles of the rhombic planes of the primitive crystal: these planes are represented at the letter *o*, in Figs. 114, 115, 116, and 121, Plate LII. of the Mineralogy lately published by the Abbé Hauy. This learned mineralogist has indeed represented a considerable increase, but not a complete one, of the above-mentioned planes, in Fig. 121, which he says was communicated to him by Mr. La Metherie. From the appearance of this form, I think it probably belongs to the tourmalins of Regensberg, in the Upper Palatinate; for many crystals of tourmalin from that place exhibit, at one of their extremities, the pyramid represented at Fig. 121 of the work just mentioned, and the pyramid I have here described, at the other. This triedral pyramid measures 107°, at the solid angle of its summit.

The second of the pyramids, is produced by the increase of the planes which have replaced the edges of the pyramids of the primitive rhomboid: these planes are represented by the Abbé Hauy at letter *n*, in Figs. 118, 119, and 120, also of Plate LII. The triedral pyramid which these planes produce, after having caused every trace of the planes of the primitive rhomboid entirely to disappear, has, very nearly, 159° for the measure of the solid angle of its summit. I have seen this variety among the tourmalins from the Ural mountains, in which, very often, the solid angle of their summit is replaced by a plane, of greater or less extent, which is perpendicular to their axis.

Among the various colours exhibited by the tourmalins which

are found in the sand of Ceylon, there are three which deserve notice, because they have not yet been mentioned by any author; these are, a light yellow, like the colour of honey, a beautiful clear emerald green, and a red slightly inclining to purple. The green variety, which indeed might easily lead to a false idea of the stone, is, most probably, what has caused some authors to mention the true emerald as being indigenous to Ceylon, where, hitherto, no trace of that stone appears to have been met with. This error was the more likely to be committed, as it was not then known that the regular hexaedral prism, with terminal faces perpendicular to the axis, was one of the crystalline forms belonging to the tourmalin; and that tourmalins of a beautiful emerald green colour, and perfectly transparent, were sometimes met with of that form. I have placed some very pretty small crystals of this kind in Mr. GREVILLE's collection.

The tourmalin of a purplish red colour, found in the sand of Ceylon, is exactly similar to that of Siberia, to which the names of *rubellite*, of *daourite*, and of *Siberite*, have been successively given, and which the Abbé HAUY has ultimately distinguished by the name of *apyrous tourmalin*. Its form is precisely the same as that of the tourmalin, properly so called; nor does the measure of its angles exhibit any difference; especially if that measure is taken upon crystals which are of a perfectly determined form, and which have not, upon their pyramidal planes, any aggregation that can cause a change in the form of those planes. I have placed in Mr. GREVILLE's collection, a small group of this kind of tourmalin, from Ceylon, the colour of which is a beautiful red; among its crystals, which have triedral pyramids with rhombic planes, may be observed one that has a dodecaedral prism, with its terminal faces perpendicular to its

axis. In Sir JOHN ST. AUBYN'S collection, I have placed a detached crystal, which has also a dodecaedral prism; one of the extremities of this crystal is of a green colour.*

Lastly, I have, in this same sand, met with a crystal, perfectly colourless, the prism of which is completely triedral;

* The scarcity of the red tourmalin of Siberia, which hitherto has been known only by very small specimens, for which the dealers demand an extraordinary price, seems to be what has hitherto prevented naturalists from forming a decided opinion respecting its proper place in the system of minerals. I am therefore happy in announcing, that there is in Mr. GREVILLE'S collection, a specimen of this kind of tourmalin, (from India,) the size and perfection of which are truly admirable. This specimen, which is not accompanied with any kind of matrix, is nearly as large as a man's head; and is entirely composed of crystals placed by the side of each other, in a diverging form, or rather penetrating each other at one of their extremities, and separating or diverging a little at the other extremity. Every one of these crystals, most of which are as long as the height of the specimen, is nearly as thick as the little finger. Their form is a hexaedral prism, which is deeply striated, and terminated by a triedral pyramid with rhombic planes, the angles of which, measure exactly the same as those of the corresponding pyramid in the common tourmalin. All the crystals are pretty transparent; and terminate on the top of the specimen, by the forementioned pyramids, but at different heights; a circumstance that gives to the top also a triedral pyramidal form, but much less obtuse than that belonging to each crystal of which it is composed. The greatest part of this specimen is of a pale purplish red, or flesh colour; but, towards the base, this colour grows much more deep, so that, at last, it becomes absolutely black. I have observed the same division of colour, in specimens of this red tourmalin from Siberia.

The superb specimen here described was brought from the kingdom of Ava: it was given by the sovereign of that country, as a present of very great value, to Colonel SYMES, who was sent on an embassy to him, by the English government. Colonel SYMES placed it in Mr. GREVILLE'S collection; and he could not pos ibly make a better use of it; that collection being, in my opinion, one of the finest in Europe, with respect to the beauty of the specimens and the instructive series of each substance which composes it, and certainly superior to all others, with respect to precious stones in a state of perfect crystallization.

The Abbé HAUY, in his Mineralogy, expresses a wish, that the prismatic enneaedral form, terminated by the triedral pyramid of the primitive rhomboid, (which he

and the pyramidal planes of which, in the only extremity of the crystal that remains, are situated upon the edges of the prism.

Ceylanite. The stone called Ceylanite, by Mr. LA METHERIE, who is the first author that has considered it as a particular and distinct species, (distinguished by the name of *pleonaste*, in the Mineralogy of the Abbé HAUY,) is also sometimes found in the sand of Ceylon; it is, however, in general, by no means common. Of the crystals of this substance that I have collected from this sand, many are perfectly transparent; a character which appears to have been hitherto unobserved in it. Its colours are very various. Besides black and green, which have already been mentioned by authors, I have seen it of a reddish or flesh colour, with a yellowish cast; of a fine bluish green, like the aqua marine; and of a fine sky blue, rather pale. When the Ceylanite is of the last-mentioned colour, whether it be a fragment or a flattened octaedron, it might very easily be mistaken for a sapphire. Its most usual colour is a brownish green.

As this substance has, in all its external characters, a striking resemblance to the spinelle, of which it is perhaps only a species, I think I cannot be too particular in pointing out those characters which may in some measure serve to distinguish it; I shall therefore add, that its hardness is rather inferior to that of the spinelle, the Ceylanite being scratched by the spinelle, while the latter cannot be scratched by the Ceylanite; also, that it usually exhibits, by irregular striæ, parallel to the edges of the regular

calls *isogone*.) may be met with in this substance, in order to determine its nature. He will no doubt feel satisfaction in hearing, that there exists, in the collection of Sir JOHN ST. AUBYN, a small detached crystal of this substance, of a fine red colour, which has exactly the above-mentioned form. This crystal I found in the sand of Ceylon.

octaedron, its primitive crystal, a tendency to the replacing of all those edges; an appearance which is very common in the octaedron of the diamond. I shall remark also, that the surface of its crystals has generally less lustre than is commonly observed in the crystals of spinelle.

The desire of contributing every thing in my power, to render as complete as possible our knowledge respecting this substance, which has been but lately known to mineralogists, induces me to add to the variety of forms that have been described by the Abbé HAUY, those represented in Figs. 53 and 54, although the Ceylanite to which those figures belong comes from a different place. The first is nothing more than the modification represented by the Abbé HAUY in Fig. 104, Plate L, of his work, but in which the four planes that have replaced each of the solid angles of the octaedron, are situated upon these same angles, in the primitive crystal itself, instead of being situated upon the planes that have replaced the edges. I have frequently seen these planes encroach upon each other, to such a degree as to render it very probable that there exists, in the Ceylanite, that form of crystal which consists of 24 trapezoidal facets, and which, by its derivation from the cube, the regular octaedron, and the regular dodecaedron, is already so very common in crystallography.

The second of the forms just spoken of, (Fig. 54.) is the same variety, but with a very slight replacement of the edges of the octaedron: it is the beginning of the change to the above-mentioned Fig. 104, of the Abbé HAUY. These two varieties belong to the Ceylanite which is inclosed in pieces of stone brought from Somma; and are indeed the most common

varieties found in them, except that in which the edges only are replaced.

Zircon. This substance is, next to the spinelle, that which is most frequently found in the sand of Ceylon. It is true, that it is generally in crystals of a very small size; but these crystals often possess the most beautiful transparency, and they are of many different colours. To the colours already mentioned as belonging to them, I may add, that they are sometimes of a reddish purple, and sometimes of a pale blue.

Lastly, if to the substances which have already been described, I add, that there are also some small scattered fragments, but in very inconsiderable quantity, of quartz, of felspar, of calcareous spar, of a brownish yellow mica, and of attractable oxide of iron, I shall have enumerated all the substances that are found in the sand of Ceylon, in the state in which it is sent to us. I have always been astonished at not finding in it any of the peridot, which, as is well known, also comes from Ceylon: hitherto, however, I have not perceived the smallest trace of it.

Of the various substances that have been here described, the spinelle is that which more particularly constitutes the sand of Ceylon, such as it comes into Europe; but it is natural to suppose, as I have already had occasion to observe, that the sand has been previously examined, and deprived of every substance, except those which are found by experience to be fit for the purposes of commerce. The other substances above mentioned, are not so constantly found in it, nor are they found always in any regular proportion. I have seen, for instance, some of this sand which did not contain an atom of perfect corun-

dum; other parcels which contained only a very small quantity; and others in which the proportion of that substance was pretty considerable: the same remark may be applied to every one of the other substances. It is therefore, I think, fair to conclude, from the above circumstances, that these sands come from different rivers or rivulets, or, if from one river only, from one into which other rivers discharge themselves; and that the nature of the sand varies, according to the particular circumstances which may have caused one or more of those rivers to bring down a greater, and others a less proportion, of the substances of which it consists. It may indeed also be asked, if what is called the sand of Ceylon comes exclusively from that island? To this question, I can give no decisive answer. I shall only observe, that the length of time it has gone under that denomination, without any alteration, gives some reason for thinking it has really some claim to it.

It is, at this time, a doubtful point, whether corundum is found in any part of the world, besides certain districts of the East Indies; although, as will presently be seen, I have strong reasons for thinking that it also exists in one of the mountainous provinces of France.

I have seen many specimens which were sent from Germany, under the name of corundum; some of them were nothing more than felspar of a brownish red colour; others were the stone called *schorlartiger beryl*, by WERNER, (the *pycnite* of the Abbé HAUY,) but in pieces which were rather less striated than is usually the case with respect to that stone.

It was thought, for some time, that a stone found at Tiree, on the eastern coast of Scotland, was of the nature of corundum.

But, after examining a specimen of that stone, which is in the British Museum, I found that its hardness, and its specific gravity, were both very inferior to those of corundum. In its exterior appearance, it very much resembles the felspar that accompanies the imperfect corundum from the Carnatic, and which I have already described, when speaking of the substances which accompany that kind of corundum in its matrix.

It is also said that corundum has been found in America, at Chesnut Hill, near Philadelphia. But there are, in the Philosophical Magazine, No. 45, for February last, some observations made by Mr. RICHARD PHILIPS, upon the external characters of the American stone, intended to show that it cannot possibly be corundum. Mr. PHILIPS has since told me, that the specimen upon which his observations were founded, was sent to him directly from Philadelphia, as a piece of the corundum found near that city. He also recalled to my mind, (which I had entirely forgot,) that he had shown me the specimen some time before; and that I then gave it as my opinion, that the crystal it contained, supposed to be corundum, was nothing more than an ill-defined crystal of quartz. Nevertheless, Mr. SMITH, a well-informed mineralogist, from America, has since assured me of the truth of the discovery of corundum, in the neighbourhood of Philadelphia. In that case, there must have been some mistake respecting the specimen that was sent to Mr. PHILIPS. Upon the whole, there still remains some uncertainty with regard to the existence of corundum in the neighbourhood of Philadelphia; and it is necessary, in order to remove all doubt on this head, either that some of the substance should be sent to us, or that some mineralogist in that country should give

such an accurate description of its characters as may serve to ascertain its real nature.

It remains for me to speak of the corundum I formerly found, or at least thought I found, in Forez, in the mountainous parts of that province which are near Montbrison. I find, by the Mineralogy of the Abbé Hauy, (Vol. IV. p. 362.) that the substance I had considered as corundum, is now looked upon in France to be of a different nature. That learned mineralogist, in the abovementioned work, seems inclined to consider it as a species of felspar, and gives it the name of *apyrous felspar*. He admits however, at the same time, that it scratches quartz; that its specific gravity is 3165; and that it is infusible by means of the blowpipe. All these characters seem to place it at a considerable distance from felspar.

The total loss of a very considerable collection of minerals, intended expressly for the purposes of study, (and which I regret the more from its having been entirely formed, and most of the specimens collected in their native places, by my own hands,) leaving me no objects of comparison, I can only consult, with regard to the above substance, the few notes I have been so fortunate as to preserve, assisting them with such circumstances as my memory has been able to retain.

I find in my notes,

First, That this substance was inclosed in a yellowish felspar, which formed a small vein in a granite rock; that, in some parts of the felspar, it appeared in the form of small spots, easily distinguishable by their colour, which was red with a purplish tinge; and that, in other parts, it was in masses of a rather larger size, from which I was able to extract some fragments.

Secondly, That the appearance of this substance was entirely different from that of felspar; and that, where it came in contact with the felspar, it seemed to mix itself with it in such an insensible manner, that, after having sawed and polished a piece composed partly of felspar and partly of the substance here spoken of, it was impossible, by the eye, to distinguish exactly where the felspar began, or, which is the same thing, where the other substance terminated.

Thirdly, I find also by my notes, that the pieces I had collected, varied considerably in their degree of hardness, although all of them were harder than felspar usually is; for many of these pieces would scarcely scratch felspar; whereas others could scarcely be scratched by the greatest number of gems or precious stones. The characters of the last mentioned or hardest pieces, appeared to me to be very similar to those of the imperfect corundum from China, a crystal of which ROMÉ' DE LISLE had sent me a short time before. The above observations, joined to the remarkable manner in which this substance is mixed with felspar, made me adopt the erroneous opinion mentioned by the Abbé HAUY, in his observations upon corundum, namely, that this substance might be nothing more than a more dense variety of felspar. I soon, however, entirely gave up this idea, after I had it in my power to examine more particularly the nature of corundum.

Fourthly, and lastly, I find by my notes, (and I also remember it perfectly well,) that among the pieces I was able, by patiently and carefully using the tools employed for that purpose by mineralogists, to extract from the vein above mentioned, there were some to which adhered small irregularly shaped pieces of a substance that was perfectly transparent, and had

a fine sapphire blue colour. The hardness of this substance was such as to be equalled only by that of the sapphire itself; and, in some of the pieces, instead of adhering to the outside, it was dispersed, in very small particles, within the interior part.

As I cannot, even at this time, consider this blue substance as any thing else than the blue perfect corundum known by the name of sapphire, I still retain the opinion I formerly thought it right to adopt, namely, that the substance to which it adhered, and which I found in the province of Forez, was really a kind of corundum. I still think also, that the variety I observed in the degree of hardness, and in the specific gravity, of different pieces, was owing to their being mixed, in various proportions, with felspar. If it should happen that, among the remains of a collection of which nothing is left to me but a painful remembrance, (although, as I have before said, my present situation is such as much alleviates my regret,) any of the specimens above spoken of still exist, and should fall into the hands of well informed naturalists, I hope they will let them serve as a basis for fresh observations. The description of the Abbé Hauy is alone sufficient to show, that the above substance cannot possibly be a kind of felspar. I am sorry, however, that he did not join to his description, the analysis of the substance; it certainly would have been very interesting, particularly if, as would most probably have been the case, the hardest pieces had been selected for that purpose.

The great difference sometimes observed in different specimens of the same substance, is exhibited in a very striking manner, in the emeralds which I found, at the same period, in a large vein of the fore-mentioned rock, but which was situated

in the part of the rock opposite to that wherein I discovered the blue substance already described. The Abbé HAUY, in his Mineralogy, (Vol. IV. page 361,) mentions these emeralds, but expresses some doubts respecting them. These doubts I think would be removed, if I had it in my power to send him the specimens I then collected. Among them were some crystals, which possessed a degree of hardness fully equal to that which is known to belong to the emerald: the hardness of many others was, however, very inferior; owing no doubt to the interposition of some heterogeneous substance, which I always suspected to be of a magnesian nature.

The Abbé HAUY, in order to fix his opinion respecting this substance, appears to require nothing but to see some crystals of it which possess the additional facets peculiar to the true emerald. I cannot indeed shew him such crystals; but I can supply the want of them, not only by my notes, but also by models cut in wood, which I was so fortunate as to bring away with me, as well as the whole collection of models of which they form a part. I find, among the models I made of these emeralds from Forez, all the varieties the Abbé HAUY has represented in Plate XLV. of his work, excepting only Fig. 50. of that Plate.

Fig. 1.

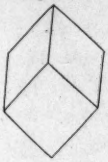


Fig. 2.

Fig. 3.

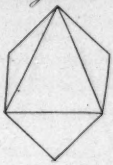


Fig. 4.

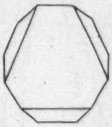
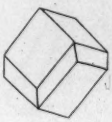


Fig. 5.



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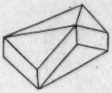


Fig. 7.

Fig. 8.

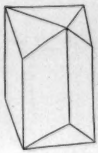


Fig. 9.

Fig. 10.

Fig. 11.

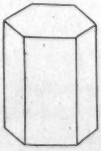


Fig. 12.

Fig. 13.

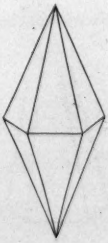
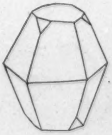


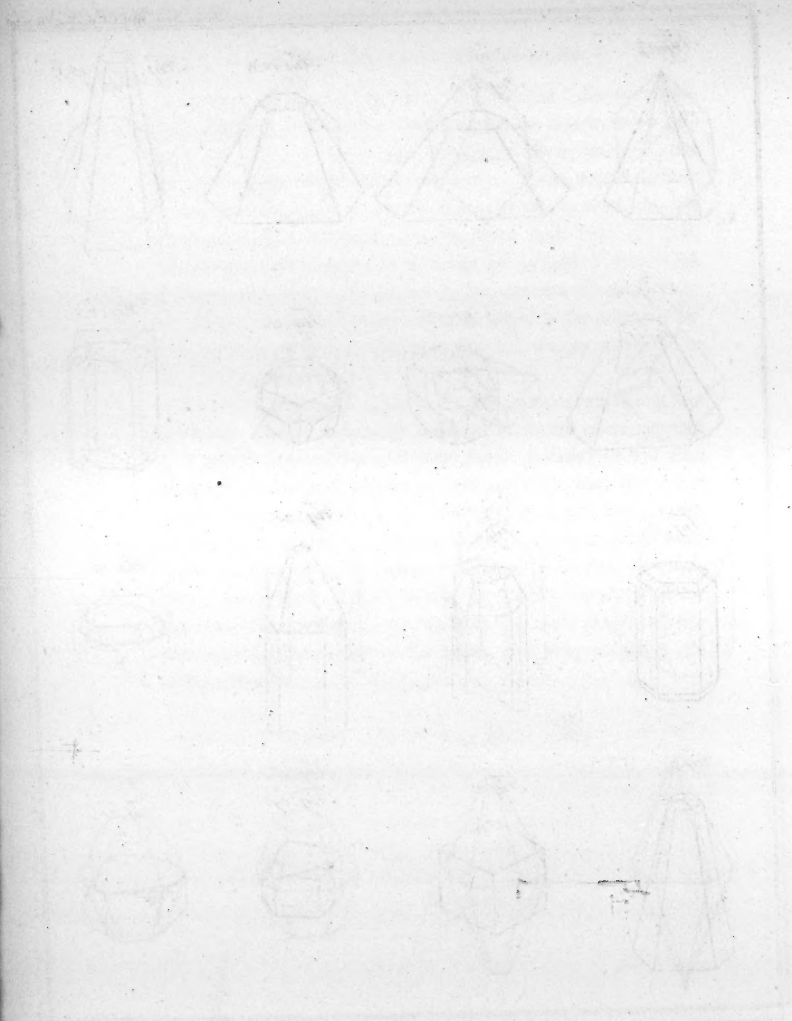
Fig. 14.

Fig. 15.

Fig. 16.



H. Basire sculpt.



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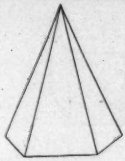


Fig. 18. A

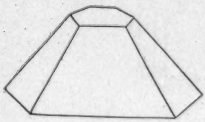


Fig. 18. B

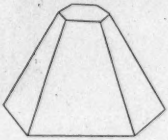
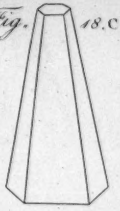


Fig. 18. C



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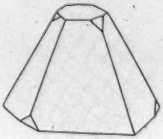

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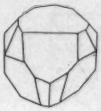
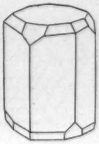


Fig. 21.

Fig. 22.



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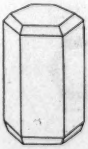

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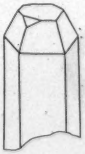


Fig. 25.

Fig. 26.

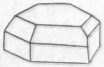


Fig. 27.

Fig. 28.

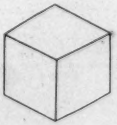
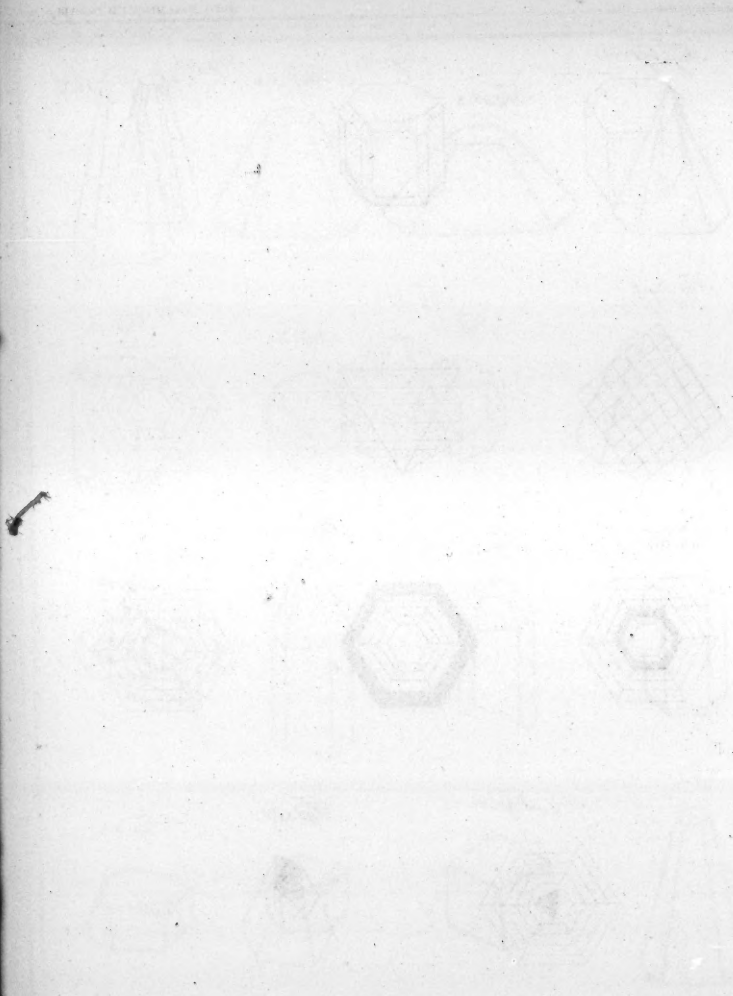


Fig. 29.

Fig. 30.



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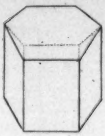

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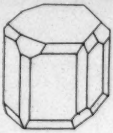
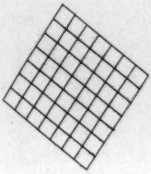


Fig. 33.

Fig. 34 A.



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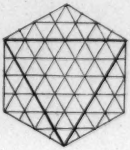

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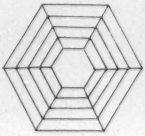

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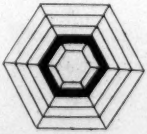

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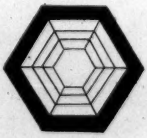

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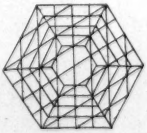
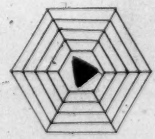


Fig. 38 A.



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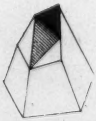


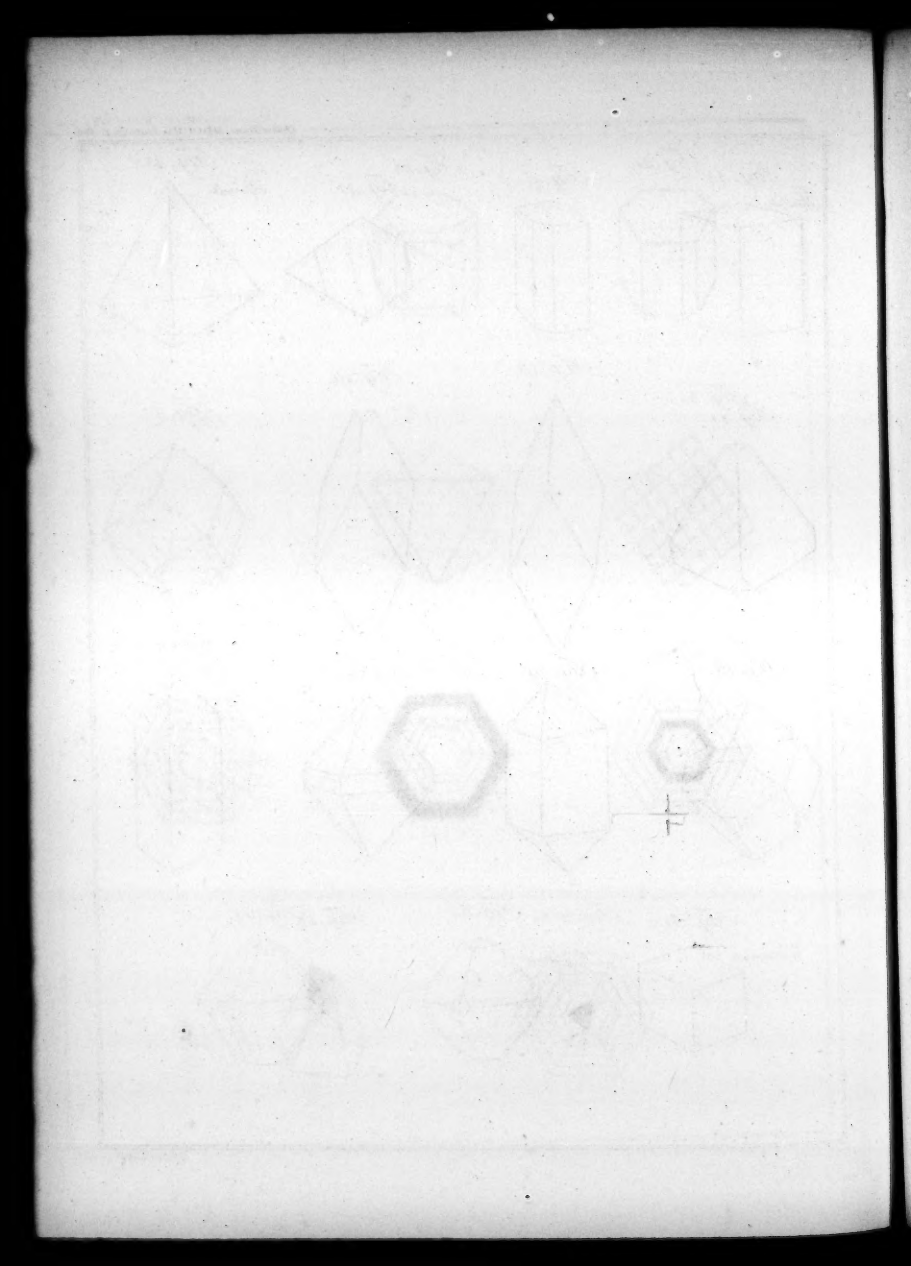


Fig. 40.
Thallite.

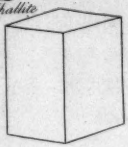


Fig. 41.

Fig. 42.

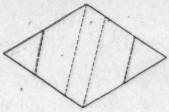
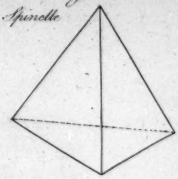


Fig. 43.
Spinelle



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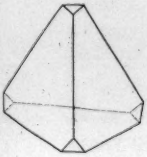
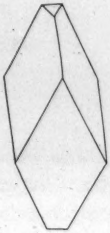


Fig. 45.

Fig. 46.



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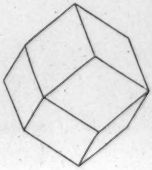


Fig. 48.

Fig. 49.

Fig. 50.

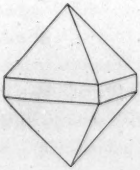


Fig. 51.

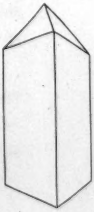


Fig. 52.
Tourmalin.

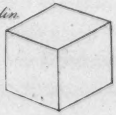
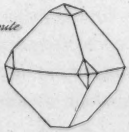


Fig. 53.
Ceylanite.



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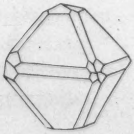


P. Basire, sculpt.

X. *Analysis of Corundum, and of some of the Substances which accompany it; with Observations on the Affinities which the Earths have been supposed to have for each other, in the humid Way.* By Richard Chenevix, *Esq. F. R. S. and M. R. I. A.*

Read May 20, 1802.

§ I.

Some kinds of corundum, such as the adamantine spar of China, and the sapphire, have already been analyzed by Mr. Klaproth. This would have rendered any further experiments unnecessary, were it not, that I have had at my disposal many kinds of corundum he did not possess, and also some substances accompanying it, which were unknown before the preceding communication of the Count de Bournon.

As, from the result of my analyses, it appears that all the different kinds of corundum are nearly similar in their constituent parts, and differ only in their proportions, it would be tedious to mention every experiment I made upon each kind. I shall therefore confine myself to stating, once for all, such modes of analysis as were employed with stones of a similar nature; and then present a summary of the results: lastly, I shall conclude with an enquiry into a much contested point, which lately threatened a revolution in docimastic chemistry.

A principal character of corundum in general, as may be found in the Count de Bournon's mineralogical description, is

extreme hardness; and thence, the difficulty of reducing that substance into fine powder will be easily conceived. We are told by docimastic chemists, that the most advantageous method of pulverizing hard stones, is to make them red hot; and, in that state, to plunge them into cold water. But I found that this operation, when performed but once, was by no means sufficient for corundum. I therefore repeated it, till the stone appeared to be fissured in every direction. After this, the specimen to be pulverized was put into a steel mortar, about three-fourths of an inch in diameter, and three inches in depth, into which a steel pestle was very closely adjusted. A few blows upon the pestle caused the stone to crumble; and the fragments were then easily reduced into an impalpable powder, in an agate mortar, with a pestle of the same material. The abrasion from the mortar, usual in the pulverization of hard stones, was much diminished by the above precaution; rubies and sapphires being, in a short time, ground to a powder nearly as minute as the finest precipitate.

Mr. KLAPROTH, in his analysis before mentioned, had observed with how much difficulty the stones were acted upon by potash or soda. I found that the greatest heat a silver crucible could support, without melting, was not sufficient to produce a satisfactory fusion of one part of corundum, with six parts of either of those alkalis; nor did an exposure to that temperature during several hours, seem to render the treatment more effectual. Not more than half the quantity of the corundum was ever rendered soluble in any acid; and what remained was the powder of the stone, wholly unchanged. The repeated filtrations and evaporations with which this treatment must be attended, not only render it tedious, but also produce

uncertainty in the results. Even when very finely powdered corundum was exposed, with six times its weight of potash, in a platina crucible, to a heat of 140° of WEDGWOOD, for two hours together, it was not acted upon in such a manner as to be fit for analysis. From all these experiments I concluded, that some more efficacious mode of rendering corundum soluble in acids was to be sought.

I boiled a great quantity of sulphuric acid upon very finely powdered corundum, in a platina crucible. But, although the acid, after a great length of time, had dissolved a little of the stone, I did not find this method more satisfactory than the others. Nitric, muriatic, and nitro-muriatic acids, were less effectual than the sulphuric. Phosphoric acid, held in fusion with corundum, did not dissolve any notable portion of that stone, or render it soluble in other acids.

I then had recourse to sub-borate of soda, (borax,) which I found to answer beyond my expectation. Two parts of that salt, calcined, and one of corundum, enter into fusion, at a temperature which I judged to be about 80° of WEDGWOOD;* and a glass, more or less coloured, is formed. This glass is soluble in muriatic acid; and, by this method, it is easy to obtain a complete solution of corundum. My general method of operating was as follows.

I took one hundred grains of corundum; and, having several times made it red hot, and plunged it into cold water, I put it into the steel mortar, and treated it as already mentioned. I then poured some very dilute muriatic acid upon it, to wash off whatever iron might have adhered, in consequence of its mechanical action upon the mortar. After it was dried and weighed,

* I have no doubt that a lower temperature would be sufficient.

I put it into the agate mortar, and ground it as fine as I could. The augmentation of weight was then noted; and was always taken into account in the general result. I then put the whole into a platina crucible, with 200 grains of calcined sub-borate of soda, and exposed the mixture for an hour or two to a violent heat. When the crucible was cool, muriatic acid was boiled upon it and its contents; and, in about twelve hours, all the glass disappeared. If I wished to obtain the silica directly, I evaporated the whole to dryness; but, if otherwise, I precipitated by an alkaline carbonate, and washed the precipitate, in order to get rid of all the salts contained in the liquor. This latter mode I believe to be preferable. I then re-dissolved the precipitate in muriatic acid, and evaporated for silica. But, as corundum contains only a small portion of this earth, there was little or no appearance of jelly. When the silica was thus precipitated by evaporation, I filtered the liquor, and boiled it with an excess of potash. By this operation, the alumina was precipitated, and then re-dissolved by the excess of potash, from which it was finally obtained by muriate of ammonia; the iron which had remained undissolved by the potash, having of course been previously separated from the alumina. This earth, and the silica, after being washed and dried, were ignited, and thus the weight of both was obtained.

I shall exemplify, in a single instance, this mode of treatment; and then present the results obtained from the different kinds of corundum. For this purpose, I shall select the blue perfect corundum, or sapphire, as the stone which has been the most ably analyzed by Mr. KLAPROTH. From a view of both analyses, the efficacy of the fusion with borax will be evident; and the results of the several experiments may be compared.

1. 100 grains of sapphire, pulverized in the agate mortar, as above stated, had increased to 105. These 105 were mixed with 250 of calcined sub-borate of soda, and put into a platina crucible. They were then exposed to a violent heat for two hours, and afterwards allowed to cool. The mass was vitrified, and had the appearance of a greenish blue glass, fissured in many directions.

2. This glass being strongly attached to the platina crucible, the whole was put into muriatic acid, and boiled for some hours. By these means, a total and limpid solution was obtained.

3. The matter of the stone was next precipitated, by ammonia not entirely saturated with carbonic acid; the liquor was filtered; and the precipitate well washed and dried. It was then redissolved in muriatic acid, and evaporated.

4. By this evaporation a precipitate was formed, which, when well washed and ignited, weighed 10,25 grains, and was silica.

5. The liquor, together with that which had washed the precipitate, was boiled in a silver vessel, with an excess of potash; this redissolved all the precipitate, except one grain.

6. Muriate of ammonia was poured into the alkaline solution. (No. 5.) The potash expelled the ammonia from the muriatic acid, and, forming muriate of potash, could no longer retain the earth in solution; a very copious precipitate, therefore, was formed. This precipitate had all the properties of alumina; and, when well washed and ignited, weighed 92 grains. Consequently, deducting 5 from the silica, for the abrasion of the mortar, we shall have for result,

Silica	5,25
Alumina	92
Iron	1
Loss	1,75
	100,00.

The chief difference between these proportions and those established by Mr. KLAPROTH, is in the silica. That chemist did not find any notable portion of it in the specimens he examined. This naturally induced me to make a very strict research, into every possible means by which any silica might have been introduced into the results; whether by the borax, the alkali, or any of the other re-agents I had used. But, finding very clearly, that none of these substances did contain any, I could no longer hesitate to believe, that the proportion I have here stated, was actually contained in the sapphire I analyzed. I am likewise convinced, that no more than the quantity I have mentioned was worn from the agate mortar and pestle; for my constant practice was, to weigh them, both before and after I had used them, in scales which, when charged with four pounds on each end, turn easily with the tenth part of a grain.

The general results, from all the different kinds of corundum, were as follows.

Blue perfect Corundum, or Sapphire.

Silica	5,25
Alumina	92
Iron	1
Loss	1,75
	100,00.

Red perfect Corundum, or Ruby.

Silica	7
Alumina	90
Iron	1,2
Loss	1,8
	100,0.

Imperfect Corundum from the Carnatic.

Silica	5
Alumina	91
Iron	1,5
Loss	2,5
	100,0.

Imperfect Corundum from Malabar.

Silica	7
Alumina	86,5
Iron	4
Loss	2,5
	100,0.

Imperfect Corundum from China.		*Imperfect Corundum from Ava.*	
Silica - -	5,25	Silica - -	6,5
Alumina - -	86,50	Alumina - -	87,0
Iron - -	6,50	Iron - -	4,5
Loss - -	1,75	Loss - -	2,0
	100,00.		100,0.

As I could not discover chrome, or any other colouring substance, except iron, in these stones, I can attribute their difference of colour only to the different state of oxidizement of the iron; but it is impossible to ascertain what that state may be, from so small a quantity.

The matrices of these stones, and the substances accompanying them, are more easily fused than the six kinds of corundum just mentioned. The usual and well known mode of treatment by potash, was sufficient to render these substances soluble in the acids. Since the many experiments of KLAPROTH, VAUQUELIN, and others, the mode of analyzing mineral bodies is become so familiar to chemists, that I shall mention particulars with respect to one only of the following substances.

MATRIX OF CORUNDUM FROM THE PENINSULA OF INDIA.

1. A certain quantity of this matrix was reduced to powder, in the manner already described. 100 grains of it were treated with potash, in a silver crucible: they then afforded a limpid solution in muriatic acid. The liquor was evaporated; and, long before the mass was entirely dry, it had assumed the appearance of a jelly. When the saline matter in the evaporating-dish was dissolved in a slight excess of acid, a white powder remained at

bottom, which had all the properties of silica, and, when washed and ignited, weighed 42,5 grains.

2. Into the liquor which had served to wash the above powder, I poured ammonia. A copious precipitate was thus formed, which was separated by filtration, and well washed.

3. Carbonate of potash also caused a precipitate in the liquor of No. 2. This precipitate was found to be carbonate of lime, and weighed 23,5 grains, = 15 of lime.

4. The precipitate of No. 2. was redissolved in muriatic acid; then boiled with an excess of potash, and filtered. There remained undissolved, 3 grains, which were iron.

5. The liquor of No. 4. was precipitated by muriate of ammonia, and afforded alumina; which, being washed and ignited, weighed 37,5 grains.

I could also perceive a trace of manganese.

The proportions therefore are,

Silica	42,5
Alumina	37,5
Lime	15,0
Iron	3,0
Loss, with a trace of manganese	2,0
	100,0.

By a similar treatment, the following substances, contained in this matrix, afforded the under-mentioned results.

Felspar.

Silica	64
Alumina	24
Lime	6,25
Iron	2,00
Loss	3,75
	100,00.

Fibrolite.

Silica	38
Alumina	58,25
A trace of iron, and loss	3,75
	100,00.

This is the only stone I have ever met with, that yielded nothing but silica and alumina; for the quantity of iron was so small as hardly to be taken into account. I have repeated this analysis three times; and have not found a difference of half a grain.

Thallite in Crystals, with a rough Surface.

Silica	45
Alumina	28
Lime	15
Iron	11
Loss	1
	100.

Thallite in Prisms like the Tourmalin.

Silica	40
Alumina	25
Lime	21,5
Iron	11,5
Loss	2
	100,0.

Thallite in Fragments, of a fine transparent Yellow Colour.

Silica	42
Alumina	25,5
Lime	16
Iron	14
Loss	2,5
	100,0.

Fibrolite accompanying the Matrix of Corundum from China.

Silica	38
Alumina	46
Iron	13
Loss	3
	100.

Felspar from the Sand of Ceylon.

Silica	68,5
Alumina	20,5
Lime	7
Iron	1,5
Loss	2,5
	100,0.

As the greater part of the above substances were fusible without difficulty in potash, I preferred using a silver crucible to any other. It may be laid down as a general rule, with respect to delicate experiments, that in the treatment of metallic substances, we should not use metallic crucibles; but, in the treatment of earthy bodies, they alone are to be depended upon. The easily oxidizable metals cannot be employed; but silver and platina present advantages which no other metals seem to possess.

Theory would certainly give a general preference to platina, from its resistance both to heat and to acids; and practice will justify this preference, in all but a single instance. If a quantity of potash be kept for some time in fusion, in a platina crucible, it will be found that the crucible has lost several grains of its weight. The platina so dissolved may be looked for in the potash; and, if this be saturated with muriatic acid, and evaporated, we shall find the well-known triple salt, formed by the combination of muriatic acid with potash and oxide of platina. This action of potash upon platina, does not depend upon any mechanical cause, such as friction, the force that determines it being purely chemical. If a salt formed by potash, or a salt formed by ammonia, be mixed with a salt of platina, a precipitate ensues, which is a triple salt; and it is by this method, that the Spanish government detects the platina, in the ingots of gold sent from their American possessions. It is therefore evident, that an affinity does exist between potash and platina, in a certain state; and I imagine it to be this affinity, which causes the oxidizement of the platina, when potash is kept in fusion upon that metal. I must however observe, that my crucible was prepared by JANETTY, in Paris, according to a method he has published in the *Annales de Chimie*; and that he always employs arsenic, a little of which certainly remains united to the platina. What influence arsenic may have, remains to be determined. Soda does not form a triple salt with the oxide of platina; for I have frequently kept this alkali in fusion, in a platina crucible, for a long time; yet very little action was produced upon the metal. This fact seems to corroborate my assertion, that the affinity of potash for oxide of platina, determines the oxidizement of the metal.

Whenever I suspected that platina had been dissolved, I could easily detect the smallest portion of it. A solution of platina, so dilute as to be nearly colourless, manifests, in a very short time, the colour of a much more concentrate solution, and becomes reddish, by the addition of a solution of tin in muriatic acid. This I have found to be, by many degrees, the most sensible test for platina; and it would answer the purposes of the Spanish government, much better than that they usually employ.

The alkalis have no immediate action upon silver; but I have observed, that crucibles of this metal, after they have been a long time in use, become somewhat more brittle than they were before.

Potash and soda have long been termed fixed alkalis; and it is certain that, if we compare them with ammonia, they are so. But *fixed* is an absolute term, and cannot admit of degrees. If potash, such as we obtain from Mr. BERTHOLLET's method of preparing it, be kept in fusion at a very strong heat, it may be totally volatilized. The vapour of the alkali may be perceived in the room; and vegetable colours will undergo the change which is usually produced by alkalis. Indeed, in preparing Mr. BERTHOLLET's potash, the vapour of the alkali may be easily perceived. Soda is not quite so volatile; though far from being fixed. It appears also, that a little water increases the volatility of both potash and soda, as happens with boracic acid. This volatility of potash, has been advantageously applied of late to the art of bleaching.

§ II.

On the Affinities the Earths have been supposed to have for each other, in the humid way.

In the course of the foregoing analysis, I had occasion to make some further observations concerning a subject upon which I had been formerly engaged, namely, on the affinities the earths have been supposed to have for each other, when held in solution by acid or alkaline menstrua.

In the XXVIIIth volume of the *Annales de Chimie*, page 189, I published a paper upon the analysis of some magnesian stones. In this paper, I took notice of the following affinities of the earths for each other, namely, the affinity of alumina for magnesia, of alumina for lime, and of alumina for silica. In the XXXIst volume, page 246, there is a memoir, by GUYTON DE MORVEAU, upon a similar subject;* and he there reports some experiments of his own, by which he was induced to think, that the earths do really possess a chemical attraction for one another. Since that time, the affinity of the earths has been received among chemists as an undoubted fact; and, at the end of Mr. KIRWAN's *Essay on the Analysis of mineral Waters*, we find a list of earthy salts which produce a reaction upon one another, supposed to be caused by an affinity that tends to unite their bases, in the form of a precipitate, insoluble in the acids. Some other detached observations are to be found, in the *Journal de Physique*, and in the *Annales de Chimie*. The fact is certainly one of the most important in the docimastic art, and merits all the attention of the skilful in that branch.

In the XLth volume of the *Annales de Chimie*, page 52,

* He has taken no notice of any of the experiments contained in my paper.

DARRACQ has published a paper, intended as a refutation of the conclusions drawn by GUYTON. I had myself repeated the greater part of the experiments of the latter; and the results I obtained were exactly similar to those of DARRACQ. In fact, I had intended to continue the researches; but the very satisfactory paper of DARRACQ, appeared to me to render a further prosecution of them totally useless. However, a paragraph inserted in the *Annales de Chimie*, (Tom. XLI. p. 206.) and of which GUYTON appears to be the author, shows that he has not derived from the Memoir of DARRACQ, that conviction which it certainly conveys. The paragraph in question is founded upon a letter, written from Freyberg, by Dr. G. M. to Dr. BABINGTON, dated December 17, 1800, and inserted in the IVth volume of NICHOLSON's Journal, page 511. This letter contains an opinion which deserves to be canvassed, as it is not perfectly just; and the use GUYTON has made of it, has determined me to add my observations to those of DARRACQ.

I shall follow the order of GUYTON's experiments, in the enumeration of those I made.

Exp. 1. From a mixture of lime-water and barytes-water, GUYTON obtained a precipitate. I obtained none.

Exp. 2. A solution of alumina in potash, mixed with a solution of silica in the same, gave a precipitate, after standing some time. This had been observed by DARRACQ, and by GUYTON, and agrees perfectly with the affinity which, before GUYTON published his paper, I had asserted to exist between these two earths.

Exp. 3, 4, 5. Lime-water, strontia-water, and barytes-water, produce a somewhat similar effect upon a solution of silica in potash.

Exp. 6. No precipitate took place from a mixture of barytes-water and strontia-water; nor from solutions of the carbonates of those earths, in water impregnated with carbonic acid.

Exp. 7. GUYTON obtained a precipitate, by mixing solutions of muriate of lime and muriate of alumina. I could not obtain any.

Exp. 8. Solutions of muriate of lime and muriate of magnesia, when mixed, did not afford a precipitate.

Exp. 9. Muriate of barytes did not, as GUYTON has asserted, form a precipitate with muriate of lime. He was right in saying, that muriate of strontia gave no precipitate with muriate of lime.

Exp. 10. Muriate of magnesia and of alumina, afforded me no precipitate. GUYTON says, that the liquors became milky.

Exp. 11. Muriate of magnesia, whether mixed with muriate of barytes or of strontia, afforded me no change; although GUYTON says he obtained an abundant precipitate, by mixing muriate of magnesia with muriate of barytes.

Exp. 12. Muriate of alumina and of barytes, did not, when mixed together, yield any precipitate. GUYTON asserts, that there is a precipitate in this case.

Exp. 13. Muriate of barytes and of strontia, did not form a precipitate. GUYTON has remarked the same.

Exp. 14. From muriate of strontia and of alumina, I obtained no precipitate. With GUYTON the liquor became milky.

From all these experiments it appears very clearly, that GUYTON has pronounced too hastily, upon the affinity which he supposes barytes to entertain for lime, for magnesia, and for alumina; and that he is equally in the wrong, with regard to the affinity of strontia and alumina. With regard to *Exp.* 3, 4, and 5, although they appear to be true, yet it would require the

respective precipitates to be further examined, before we admit a decided affinity between the earths. The quantity of carbonic acid also, which must of course combine with the potash, during the treatment of the silica by that alkali, should be taken into account, in considering the cause of the precipitate.

The solutions which I used, of all the above salts, were in the most concentrate state; therefore, in the state most favourable for showing precipitation, if any had taken place.

It is not very difficult to account for the appearances that deceived Mr. GUYTON in his experiments, and for the cause that produced them. In one instance, he obtained a precipitate from muriate of lime and of alumina, because, in all probability, the alumina he dissolved in muriatic acid had been precipitated from alum; and alumina, thus prepared, retains a small portion of sulphuric acid.* In the next place, it is very likely that his solutions were sufficiently concentrate to give a precipitate of sulphate of lime. The same was the case with regard to his mixture of muriate of strontia with muriate of alumina. As to the general conclusion, that barytes has an affinity for lime, magnesia, and alumina, which strontia does not appear to possess, it is to be explained as follows. Lime often contains a little sulphate of lime. Mr. GUYTON's magnesia, as well as his alumina, had probably been obtained from the sulphate; and we are indebted to Mr. BERTHOLLET, for the true nature of many similar precipitates.

* It is somewhat singular, that GUYTON should have observed this fact elsewhere. See his experiments on the diamond, in the *Annales de Chimie*. The preparation of a barytic salt, by alumina prepared from the sulphate of this earth, had been observed by SCHEELE, in his *Essay on the Affinities of Bodies*. But that great chemist referred the phenomenon to its right cause, viz. to some sulphuric acid remaining in all alumina thus prepared.

Barytes is a much more delicate test than strontia, for sulphuric acid; and, therefore, barytic solutions were affected by quantities of sulphuric acid, which strontia could not render sensible. This I have ascertained to be the case: for I have obtained copious precipitates, by barytes, in a liquor composed for the purpose, wherein strontia did not produce the smallest cloud, or show the presence of sulphuric acid.

A little care and attention are necessary, in preparing the earths, which are to be dissolved in the muriatic acid, for these experiments; and, if Mr. GUYTON had taken the requisite precautions, he would not have been led into error. The object to be kept in view is, to free the earth from sulphuric acid; and, if this be obtained, there is not the smallest precipitate or cloud, in any of the cases I have mentioned. If any further proof be necessary, with regard to the cause of precipitates obtained in the manner stated by Mr. GUYTON, I may add, that I have repeated his experiments, and have always found the precipitates to be sulphate of barytes.

The general conclusion to be drawn from the observations of Mr. KIRWAN, already alluded to, is, that barytes has an affinity for lime, magnesia, and alumina, upon which earths strontia does not seem to have any influence. But these mistakes are to be accounted for in the same manner as those of Mr. GUYTON, viz. by sulphate of barytes being much less soluble than sulphate of strontia, and therefore showing the presence of a smaller portion of sulphuric acid, or, in other words, being a much more delicate test for that substance.

With regard to the letter already mentioned as being inserted in NICHOLSON's Journal, and which drew some reflections from

Mr. GUYTON, it is necessary to examine as much of it as may be thought objectionable.

The author says, that he repeated the experiments of Mr. GUYTON, with an alkaline solution of silica and alumina, and that he obtained a precipitate; which precipitate, though containing silica, was totally soluble in the acids. "Here," he says, "the properties of the silex must be considerably altered. This "must render all analysis with alkalis suspicious; and shows on "what fallacious grounds the proud dominion of chemistry rests, "which she has exercised so long, in such an arbitrary and over-"bearing manner, in the mineral kingdom." This opinion is by no means likely to overthrow the pretensions of chemistry; for the very circumstance of rendering silica soluble in the acids, is one of the discoveries that has most contributed to render certain, and to extend, our knowledge of analysis. No earthy substance is now thought fit to be submitted to further experiment, till a complete solution of it in an acid be first obtained; and, when that solution cannot be effected directly by the acid, it is always attempted by previous fusion with an alkali. This mode of rendering silica soluble in acids, is no new discovery; it has been long known; and the analysis of minerals has never been brought so near to truth, as since it has become an indispensable condition.

I have no doubt as to the fact of a precipitate being formed, by mixing together an alkaline solution of silica and alumina. Alumina indeed appears to exercise an attraction, as I before stated, for silica, for magnesia, and for lime. All stones in which there is but little alumina, and a great quantity of silica, leave, after fusion with potash, a light and flocculent substance, which

cannot be dissolved by the acids: this substance, however, which is silica, has been in solution in the alkali. But, if a greater proportion of alumina be present, none of this flocculent precipitate appears; hence it is evident, that alumina must determine its solution. Its easy solubility, in the latter case, cannot depend upon the division of the particles of the silica in the stone; for, in the first place, after being fused with potash, the tenuity of the particles of every stone must be nearly the same; and, in the next place, I have not observed, that any earth, except alumina, can promote the chemical solution of the silica, though they must all occasion its mechanical division.

As to the affinity of alumina for magnesia, it is by much the most powerful of all those which any of the earths have for each other. I attempted to precipitate magnesia from muriatic acid, by ammonia, even in excess; but found that the whole muriate of magnesia had not been decomposed, and that a triple salt, or an ammoniacal muriate of magnesia,* had been formed. I then poured an excess of ammonia into a solution of muriate of magnesia, mixed with a large proportion of a solution of muriate of alumina. All the earth was precipitated; and nothing remained in solution, except muriate of ammonia. The liquor was then filtered, and the precipitate washed and dried. I dissolved it in muriatic acid, and boiled it with a great excess of potash. Some alumina was taken up, but by no means all the quantity that had been used. The precipitate which had resisted the action of potash, was again dissolved in muriatic acid, and precipitated by carbonate of potash. The carbonate of magnesia was held in solution by the excess of carbonic acid; and, by using potash and carbonic acid alternately, (the first to

* This salt is well known in chemistry.

dissolve alumina, the second to dissolve carbonate of magnesia,) I effected a separation of the earths. These experiments show, that there is an affinity between alumina and magnesia, and a certain point of saturation, where the action of potash upon alumina is wholly counteracted by the affinity of that earth for magnesia.

When a solution of potash is boiled upon a mixture of lime and alumina, the alumina is dissolved, together with a much greater portion of lime than can be attributed to the dissolving power of the water alone. But, if a solution of potash be boiled upon lime, without alumina, no more lime is taken up than would have been dissolved by an equal quantity of water not containing potash in solution; consequently, alumina seems really to promote the solution of lime in potash. The affinity of alumina for lime, I had mentioned in the paper to which I allude; and it has since been noticed by Mr. VAUQUELIN.*

If the conclusions of Mr. GUYTON had been well founded, it would have been chemically impossible to arrive at truth in analysis. There were already real difficulties enough to be overcome; and Mr. BERTHOLLET has lately discovered some, which are not so easily answered as those I have just considered. The position of this chemist, however, has been too generally extended by him. If the power of masses were as great as he represents it to be, and if it increased *ad infinitum*, in proportion to the mass, it must follow, that, with any given substance, we could decompose any compound, provided the mass of the decompounding body were sufficiently great; but this is well known not to be the case.

* SCHEELE was, in fact, the first who perceived this affinity. See his *Essay on Silex, Clay, and Alumina.*

From the experiments which I have related, it appears to be proved.

1st. That there exists an affinity between silica and alumina.

2dly. That there exists a very powerful affinity between alumina and magnesia.

3dly. That alumina shews an affinity for lime; but that the said affinity is not so strong as Mr. GUYTON had supposed, nor, if pure reagents be used, is it to be perceived under the circumstances stated by him.

4thly. That Mr. GUYTON was mistaken in every instance of affinity between the earths, excepting in the case of silica with alumina, which had been observed before his experiments; and that, in the other cases, he has attributed to a cause which does not exist, phenomena that must have resulted from the impurity of his reagents.

5thly. That neither the experiments of Mr. GUYTON, nor the opinion maintained in the letter from Freyberg, are sufficient to diminish, in any degree, the value of the assistance mineralogy derives from chemical investigation.

XI. *Description of the Anatomy of the* Ornithorhynchus Hystrix. *By* Everard Home, *Esq. F. R. S.*

Read June 3, 1802.

At the time I had the honour of laying before this learned Society, an anatomical description of the Ornithorhynchus paradoxus, (see page 67,) I did not attempt to point out any quadrupeds as being nearly allied to it, there being none at that time within my knowledge; but the discovery of another of the same tribe, which is the subject of the present Paper, enables me to trace one step further, in the gradation between that extraordinary animal and the more perfect quadruped.

The subject from which the following description was taken, was sent from New South Wales, preserved in spirit. It is a male, and had arrived nearly at its full growth, as the epiphyses were completely united to the bodies of the bones, which is not the case in growing animals.

A description and figure of this animal is given by Dr. Shaw, in his Zoology, under the name of *Myrmecophaga aculeata*.

Description of the external Appearances.

The animal is 17 inches long, from the point of the bill to the extremity of the tail; the bill is $1\frac{3}{4}$ inch long, and the tail half an inch.

The body of the animal is nearly of the same general thick-

ness, but rather larger just below the shoulders. The greatest circumference of the body is 17 inches.

The back and sides are covered with short coarse hair, half an inch long, and with quills like those of the porcupine, only shorter and less pointed; they appear to be ranged in rows, in the direction of the animal's length; those on the sides are $2\frac{1}{2}$ inches long, the others between one and two inches. The quills on each side of the body, between the setting on of the hind legs and the tail, have a direction forwards, so as to be opposed to the others.

The head and neck are covered with a coarser hair than the rest of the body, and are almost entirely without quills.

On the breast, the hair is long and soft, and without quills; on the skin of the belly, it is almost entirely wanting.

No appearance of false nipples could be detected, either on the belly or breast.

Externally there is no appearance of organs of generation; the orifice of the anus being a common opening to the rectum and the prepuce of the penis.

The bill, which projects from the head in a tubular form, is $1\frac{3}{4}$ inch long. It is conical in its shape, convex upon the upper surface, and flat upon the lower; at its point, it is $\frac{1}{8}$ of an inch in diameter, and $\frac{7}{8}$ at its base: it has the same smooth cuticular covering as the bill of the Ornithorhynchus paradoxus, but has not the lateral lips, the sides being closed to within half an inch of their extremity. The upper part of the bill is formed by an elongation of the nose and palate; and the lower portion by a continuation of the two bones of the under jaw, as in the paradoxus.

The nostrils are two small orifices, close to each other, within a quarter of an inch of the end of the bill.

The eyes are very small, and are situated laterally on the head, close to the base of the bill.

The external ears are two oval slits, an inch long, situated nearer to the upper part of the head than the eyes, and $2\frac{1}{2}$ inches further back.

The teeth, if they can be so called, being, like those of the paradoxus, composed of a horny substance, and not of ivory and enamel, as in all other quadrupeds, are not situated on the margin of the palate and lower jaw, but are confined to the tongue and surface of the palate. On the posterior part of the tongue, which is thicker and broader than the rest, there is a space, one inch in length and $\frac{3}{4}$ broad, covered with a strong cuticle, and having about 20 small teeth, blunt at their ends, projecting about $\frac{1}{10}$ of an inch; there are also several others, less prominent. On that part of the palate immediately opposite, there are seven transverse rows of very slender horny teeth, with their points directed backwards: each row looks somewhat like a small-toothed comb, laid flat upon the palate.*

The appearance of these horny teeth, and a general view of the palate and tongue, are represented in Plate XI.

The fore legs are short and thick, and have five toes, with strong blunt claws, intended probably for the purpose of digging; the middle claw is the longest, the others becoming gradually shorter. The leg, to the end of the longest claw, is

* In the duck, both upon the tongue and palate, there are horny papillæ, which have a slight resemblance to the horny teeth just described; those on the tongue are lateral, six on each side.

three inches long; the palms of the feet are covered with a strong cuticle.

The hind legs are longer than the fore legs, and have five toes; four of these have long strong claws, the innermost is the longest. The fifth toe is short, and, being opposed to the others, resembles a thumb. The length of the leg, to the point of the longest claw, is six inches. Just at the setting on of the heel there is a spur, similar to that of the paradoxus, only weaker and smaller; it is $\frac{1}{8}$ of an inch long.

The tail is covered with hair, and is about half an inch in diameter; it terminates in a blunt end.

Description of the internal Parts.

The internal structure so nearly resembles that of the Ornithorhynchus paradoxus, that a particular description of many of the parts will be unnecessary.

The panniculus carnosus is similar to that of the paradoxus. The tongue is cylindrical, very small towards the point, and eight inches long. Near the root there is an oval portion, more massy than the rest, on which are placed the horny teeth already described.

The velum pendulum palati, and glottis, resemble those of the paradoxus; but, at the termination of the fauces in the œsophagus, there is a projecting fold or valve, peculiar to this species; and the epiglottis is bifid in a small degree.

In the structure of the bones of the chest, there are the same general peculiarities as in the paradoxus; but, in the Hystrix, there is a xiphoid cartilage, having its origin from the under surface of the sternum, and being about one inch in length.

The heart and lungs, both in their structure and relative situation, resemble those of the paradoxus, with the exception of the heart having only one vena cava superior, instead of two.

The diaphragm is similar to that of the paradoxus.

The œsophagus is small, but has several longitudinal folds, which render it capable of dilatation; it is lined with a strong cuticle, which is continued down to the cavity of the stomach.

The stomach is a thin membranous bag, nearly of the shape of the human stomach; in its collapsed state, it measured $4\frac{1}{2}$ inches in length, and 3 inches in breadth.

Its internal membrane is smooth, and without the appearance of glands, except towards the pylorus: it is lined with a cuticle; and the glandular part has horny papillæ, $\frac{1}{10}$ of an inch long, which appear to be the excretory ducts through which the gastric juice is conveyed into the cavity. This uncommon appearance is represented in Plate XI.

Similar cuticular papillæ are to be observed in the paradoxus; but they are so extremely small as to require a particular examination to detect them: the stomach of that animal also appears to be lined with a thin cuticle.

Along with the food, a quantity of sand is received into the stomach, and passes down through the bowels; it was met with in different parts of the small intestines, and also in the colon; it was very fine, and of a white colour.

It is deserving of observation, that in this animal, the mode of managing the food is different from that employed in the paradoxus; which accounts for the difference in the appearance both of the teeth and stomach.

In this species, the food is bruised between the teeth placed upon the tongue and those of the palate; and, immediately after-

wards, the whole is conveyed into the stomach, and along with it a quantity of sand.

The stomach therefore is sufficiently large to contain the food, and the extraneous matter connected with it; and is defended from injury by its cuticular lining. In the paradoxus, the food is received into the mouth, is retained in the lateral pouches, and is prevented, by the two projecting teeth on the tongue, from getting into the stomach, till all the indigestible parts are separated; the nutritious matter alone being allowed to reach the stomach, which is of a very small size.

The course of the intestines, and the form of the cæcum, are the same as in the paradoxus; the cæcum is shorter, being only half an inch long.

The small intestines are seven feet, the colon and rectum two feet long.

The rectum is similar in every respect to that of the paradoxus.

The mysentery, its glands, and the lacteals, are also similar to those of the paradoxus.

The internal membrane of the duodenum has a corrugated appearance, but no valvulæ conniventes. The cavity of the small cæcum is not loculated; and there are ten or twelve excretory ducts of glands on the membrane of the colon, near the opening of the cæcum; but these are placed irregularly; and there are many similar orifices, in different parts of its course.

The liver and gall-bladder, with their ducts, and also the omentum, are similar to those of the paradoxus.

The pancreas is not so much separated into detached parts as in the paradoxus; but is less compact than in quadrupeds in general.

The spleen is shorter and thicker than in the paradoxus; but has the same general shape.

The kidneys and bladder are exactly similar to those of the paradoxus.

The skull, in its general shape, is similar to that of the duck; and has not the bony falciform process observed in the paradoxus.

The brain was not in a state to admit of particular examination.

The olfactory nerves are divided into numerous branches.

The optic nerves are small; and the fifth pair of nerves is much smaller than in the paradoxus; the second branch, which in that species is very large, and supplies the upper part of the bill, is either extremely small, or altogether wanting. This animal has therefore, probably, a less acute sense of feeling in the bill than the paradoxus; and, as the organ of smell is more complex, the increase of that sense may make a nice discrimination by touch less necessary.

The eye-lid is very loose upon the eye-ball, has a circular aperture, and appears to have great extent of contraction and relaxation. The membrana nictitans is wanting.

The eye-ball is $\frac{1}{20}$ of an inch in diameter; the cornea $\frac{1}{20}$, surrounded by a zone of a black pigment, $\frac{3}{20}$ in breadth.

The organ of smell differs materially from that of the paradoxus. Immediately below the cribriform plate of the ethmoid bone there are bony processes, forming a cellular structure, nearly half an inch thick, which constitutes the principal part of the organ; from this there is a convex projecting turbinated bone, of a very slender form, extending half way to the external opening of the nostril, with a corresponding concave one to receive it, in each nostril; and there is a small slit or

opening between the two nostrils. The structure of the organ is shown in Plate XI.

The external opening of the ear is large enough to admit the end of the finger; the meatus takes the same sweep as in the paradoxus; just before it reaches the membrana tympani, it contracts to the size of a crow-quill, then again dilates, forming a cavity round the membrana tympani: it is lined with hair, till it forms this constriction.

The membrana tympani is externally concave, and is covered by a cuticle. It is of an oval form; the long axis of the oval is $\frac{4}{20}$ of an inch, the short one $\frac{3}{20}$. Its centre is attached to a small bone, connected with the bony rim by which the circumference of the membrane is supported: this bone corresponds to the malleus of the quadruped. On the inner side of this, and united to it by a smooth surface, is a small bone, in the form of a trumpet, which may be considered as the stapes, as it fills the opening of the foramen ovale.

There is no perfect cochlea, as in quadrupeds in general; but there is the imperfect cochlea met with in the bird, which has been accurately described by Mr. Cuvier.* It consists of a conical cavity, a little bent, in the middle of which there is a double cartilaginous septum: the two laminæ unite before they reach the end of the cone; by this means, the surrounding cavity becomes a spiral canal, one end of which opens into the vestibulum, the other terminates at the foramen rotundum.

The male organs of generation bear a close resemblance to those of the paradoxus. The testicles are in every respect similar: the vasa deferentia open into the urethra, close to the neck of the bladder, as is seen in Plate XII. and it is at the same part they open in the paradoxus.

* *Leçons d'Anatomie comparée.* Vol. II. p. 464.

The urethra for the urine opens into the rectum, about an inch from the anus; and the passage for the semen goes into the penis, in the same manner as in the paradoxus.

The penis is very elastic in its substance; when drawn out, it is about three inches long; but, from having been so long kept in spirit, is not sufficiently ductile to allow of an accurate judgment respecting its real length. The glans is externally subdivided into four equal processes; in the centre of each of these is an orifice, surrounded by concentric circles of infinitely small prominent papillæ.

There is a gland on each side of the rectum, the size and situation of which are delineated in Plate XII.; each of these has a small excretory duct, which passes to the root of the penis, where they unite, and then open by one common orifice into the urethra for the semen, $\frac{1}{10}$ of an inch after it has entered the penis.

These glands must be considered as corresponding to COWPER's glands in the human subject, and not as a substitute for the prostate gland, or the vesiculæ seminales, since something analogous is met with in the female.

In my account of the Ornithorhynchus paradoxus, these glands are described as belonging to the rectum. This mistake arose from the parts being so much coagulated, by long continuance in strong spirit, as to make it impossible to distinguish the excretory duct from the surrounding blood-vessels, or other parts. In the specimen of the Hystrix from which this description is taken, the parts were in the same state, and would have led me into a similar error, had I not been favoured by Sir JOSEPH BANKS with a specimen of the paradoxus, brought from New South Wales by Mr. BELMAIN, which had

been kept in weak spirit; and, although many other parts had become putrid, those connected with the organs of generation had been preserved, and were in a flaccid state, more favourable for anatomical examination.

I was not only enabled to examine these glands and their ducts, but also, by fixing a pipe into the urethra where it enters the penis, to inject water along that canal, so as to make it fill a small cavity in the centre of each glans, and from that pass through all the papillæ, which became erect as soon as the glans was turgid, and scattered the water by so many small streams, about the size of a horse-hair, in every direction.

Upon re-examining the female organs of the paradoxus, after they had been steeped in water, I was enabled to trace the ducts of the glands, which correspond with those of the male, to one common orifice on the posterior surface of the vagina, $\frac{1}{4}$ of an inch within the orifice of that canal.

A clitoris was also detected, with two crura, arising from the outer side of the common vestibulum to the rectum and vagina. The clitoris was very slender, half an inch long; its glans a little bifid, and inclosed in a thin prepuce; the end of the glans only projected into the vestibulum.

The female organs of the Hystrix have not been examined; but there can be no doubt of their bearing the same resemblance to those of the male as in the paradoxus.

Another species of Ornithorhynchus, of the same size as the Hystrix, was shot at Adventure Bay, Van Diemen's Land, by Lieutenant Guthrie, in the year 1790, a drawing of which was made by Captain Bligh, and sent to Sir Joseph Banks, who has allowed me to annex a copy of it to this Paper. The quills

of this species, as I am informed by Captain BLIGH, are so short, that the points only are seen projecting beyond the hair.

The Ornithorhynchus Hystrix is a nearer approach to the more perfect quadruped than the paradoxus; and, as its tongue is similar in some respects to those of the Manis and Myrmecophaga, it was natural to look among the different species of these genera, for other points of resemblance.

I have examined a figure of the Manis of Sumatra, drawn by the late Mr. BELL, while resident there, whose abilities as an anatomist and draughtsman, make his death a considerable loss to science.* The form of the head, the opening of the mouth, and the general appearance of the animal, led me to believe it a still further remove from the Ornithorhynchus than the Myrmecophaga; and the following circumstances, in the internal structure of these two genera, confirm this opinion. The Myrmecophaga has two cæca, which resemble that of the Ornithorhynchus; whereas the Manis has no appearance whatever of cæcum.

There are two specimens of Manis preserved in spirit, in the HUNTERIAN Museum, one male, the other female; both of these I have examined.

The tongue was small, cylindrical, and very long; and the muscle by which it is retracted lay between the abdominal muscles and peritonæum of the right side, forming a semicircle between the lower end of the sternum and the navel: the theca in which it was inclosed, had an attachment to the lower end of the sternum. The tongue was smooth; and there was no appearance of teeth on it, or on the palate.

* This drawing is in the possession of Mr. MARSDEN, who proposes publishing it in the next edition of his History of Sumatra.

There was no cæcum, the intestine suddenly enlarging to form the colon: on each side of the anus there was a bag, as in the otter, and most other animals which have no cæcum.

The organs of generation, in both sexes, were distinct from the anus; the penis was small. In the female there were two nipples upon the breast. The uterus was broad at its fundus; and the two horns separated from each other, nearly at right angles to the middle line of the uterus.

The didactyla is the only species of Myrmecophaga which has come under my observation. The Trustees of the British Museum allowed me, in the most liberal manner, to examine both the male and female. The tongue had a general resemblance to that of the Ornithorhynchus Hystrix; but there were no cuticular teeth upon it, or on the palate. The cæcum was of the same kind, but double, and each of them was only $\frac{1}{8}$ of an inch in length. In the other parts there was no similarity. The male had four false nipples, two on the breast and two on the belly, corresponding with the true nipples of the female.

The organs of generation were not connected with the rectum. The uterus was nearly of the shape of the human uterus; its coats were very thin; and the cavity larger in proportion than in most quadrupeds. There were no horns; and the fallopian tubes went off from the posterior part. This is an approach to the uterus of the Opossum.

With a view to procure information respecting the other species of Myrmecophaga, I wrote to Mr. Cuvier of Paris, whose abilities and extensive researches in comparative anatomy, have so deservedly distinguished him in that branch of science.

By a letter from him, I find that the Myrmecophaga jubata, Tamandua, and capensis, belong decidedly to the class Mammalia;

and therefore are not so nearly allied to the Ornithorhynchus as I had at first been led to imagine. The Myrmecophaga jubata, which is described by Mr. ZAN to have the organs of generation, in both sexes, concealed within the verge of the anus, appears to be a nearer approach to it than the other species.

The peculiar characters of the Ornithorhynchus, as a genus, or more properly a tribe of animals, are,

The male having a spur on the two hind legs, close to the heel.

The female having no nipples.

The beak being smooth, while the rest of the animal is covered with hair.

The tongue having horny processes, answering the purposes of teeth.

The penis of the male being appropriated to the passage of the semen; and its external orifice being subdivided into several openings, so as to scatter the semen over an extent of surface, while the urine passes by a separate canal into the rectum.

The female having no common uterus; and the tubes which correspond to the horns of the uterus in other quadrupeds, receiving the semen immediately from the penis of the male.

These characters distinguish the Ornithorhynchus, in a very remarkable manner, from all other quadrupeds, giving this new tribe a resemblance in some respects to birds, in others to the Amphibia; so that it may be considered as an intermediate link between the classes Mammalia, Aves, and Amphibia; and, although the great difference that exists between it and the Myrmecophaga, the nearest genus we are at present acquainted with, shows that the nicer gradations towards the more perfect quadrupeds are not at present known, the facts which have

been stated may induce others to prosecute the inquiry, and render that part of the chain more complete.

Between it and the bird, no link of importance seems to be wanting.

The great affinity between the male organs of the Ornithorhynchus and those of birds, is best illustrated by comparing the penis of the former with that of the drake, a figure of which is annexed. (Plate XII. Fig. 2.) It is six inches long when drawn out to its full extent; but, when left to itself, (so great is the contractile power of the urethra,) it retracts, and confines the whole penis within the verge of the rectum.

The urethra begins by a blunt end; and the vasa deferentia open into it close to its origin: its sole use, as in the Ornithorhynchus, is to eject the semen.

When more of this extraordinary tribe of animals, which, although quadrupeds, are not Mammalia, shall have been discovered, and naturalists thereby enabled to divide them properly, the two which I have described will doubtless be arranged under different genera; till then, I have thought it best to consider them as species of the same genus, rather than encumber science with an additional name, or attempt to frame generic characters from one species only.

PLATE X.

A figure of the Ornithorhynchus Hystrix, (on a scale of half an inch to an inch,) to show its general appearance, but more particularly its cuticular bill.

PLATE XI.

Fig. 1. A view of the bill and throat, laid open, to show the tongue and palate.

a. The tongue in its natural situation.

b. The cuticular teeth upon the tongue.

c. The cuticular teeth upon the palate.

d. The bifid epiglottis immediately above the glottis.

e. The valvular projection at the beginning of the œsophagus.

Fig. 2. A section of the nose and skull, to show the peculiarities of the organ of smell, and the shape of the cavity of the skull, in which the bony falx met with in the paradoxus is wanting.

a. The cavity of the skull.

b. The peculiar structure of bone through which the branches of the olfactory nerve pass, after leaving the cavity of the skull.

c. The turbinated bone, or what corresponds to it.

d. The septum of the nose.

e. The slit through the septum.

f. The posterior nostrils.

Fig. 3. The appearance of cuticular papillæ on the internal

membrane of the stomach, situated at the termination of the pylorus in the duodenum.

Plate XII.

Fig. 1. The penis and testicles in their relative situation, to show the urethra for the passage of the urine, and that for the semen.

aa. The glans penis divided into four projecting processes, which in the relaxed state are concave; the orifice is in the centre of each of the projections.

b. The body of the penis.

cc. The rectum laid open.

dd. The orifices of the glands of the rectum.

ee. The two glands which correspond to Cowper's glands, their excretory ducts opening into the urethra of the penis.

f. The termination of the urinary urethra in the rectum.

g. The urethra laid open through its whole course.

h. The opening leading to the urethra for the semen.

i. The orifice of the neck of the bladder.

k. The urinary bladder.

ll. The openings of the vasa deferentia into the urethra.

mm. The bodies of the testicles.

nn. The epididymis of the testicles.

Fig. 2. The penis of the drake, in its extended state.

aa. The verge of the fundament surrounded by the feathers.

bb. The urethra laid open through its whole extent.

cc. The orifices of the vasa deferentia.

dd. The prepuce of the penis laid open, and, from its elasticity, thrown into serpentine folds.

PLATE XIII.

Another species of Ornithorhynchus, 17 inches long, with small quills, about one inch long. The animal, when it walked, had its body raised about two inches from the ground. It was shot at Adventure Bay, Van Diemen's Land.

XII. *A Method of examining refractive and dispersive Powers, by prismatic Reflection.* By William Hyde Wollaston, *M.D. F.R.S.*

Read June 24, 1802.

In examining the power with which various substances refract and disperse light, I have for some time past employed a method unnoticed by writers on optical subjects; and, as it is not only convenient in common cases of refraction, but also capable of affording results not attainable by other means, I have been induced to draw up a short account of the method itself, and of the most remarkable instances of its application.

This method was suggested by a consideration of Sir Isaac Newton's prismatic eye-glass, the principle of which depends on the reflection of light at the inner surface of a dense refracting medium.

Since the range of inclination within which total reflection takes place, depends not only on the density of the reflecting prism, but also on the rarity of the medium adjacent to it, the extent of that range varies with the difference of the densities of the two media. When, therefore, the refractive power of one medium is known, that of any rarer medium may be learned, by examining at what angle a ray of light will be reflected from it.

For instance, when any object is laid under a prism of flint-glass, with air alone interposed, the internal angle of incidence at which the visual ray begins to be totally reflected, and at which

the object ceases to be seen by refraction, is about 39° 10′; but, when the object has been dipped in water, and brought into contact with the glass, it continues visible, by means of the higher refractive power of the water, as far as $57\frac{1}{2}°$ of incidence. When any kind of oil, or any resinous cement, is interposed, this angle is still greater, according to the refractive power of the medium employed; and, by cements that refract more strongly than the glass, the object may be seen through the prism, at whatever angle of incidence it is viewed.

In examining the refractive powers of fluids, or of fusible substances, the requisite contact is easily obtained; but, with solids, which can in few instances be made to touch to any great extent, this cannot be effected without the interposition of some fluid, or cement, of higher refractive power than the medium under examination. Since the surfaces of a stratum so interposed are parallel, it will not effect the total deviation of a ray passing through it, and may therefore be employed without risk of any error in consequence.

Thus, resin, or oil of sassafras, interposed between plate glass and any other prism, will not alter the result.

If, on the same prism, a piece of selenite and another of plate-glass be cemented near each other, their powers may be compared with the same accuracy as if they were both in absolute contact with it.

For such a mere comparison of any two bodies, a common triangular prism is best adapted; but, for the purpose of actual measurement of refractive powers, I have preferred the use of a square prism, because, with a very simple apparatus, it shows the sine of refractive power sought, without the need of any calculation.

Let A, Fig. 1, Plate XIV. be a square or rectangular prism, to which any substance is applied at *b*, and let any ray of light parallel to *cb* be refracted through the prism, in the direction *bde*.

Then, if *ef* and *ed* be taken proportional to the sines that represent the refractive powers of the prism and of air, *fg*, which is intercepted between *f* and the perpendicular *eg*, will be the corresponding sine to represent the refractive power of the medium *b*. For, since *edg* (opposite to *ef*) is the angle of refraction, *efg* (opposite to *ed*) must be equal to the angle of incidence *bdb*; and *ef* : *fg* :: *bd* : *db* :: sine of *cbi* : sine of *bbd*.

All therefore that is requisite for determining the refractive power of *b*, is to find means of measuring the line *fg*. On this principle, the instrument in the annexed sketch (Fig. 2.) is constructed. On a board *ab* is fixed a piece of flat deal *cd*, to which, by a hinge at *d*, is jointed a second piece *de*, 10 inches long, carrying two plane sights at its extremities. At *e* is a second hinge, connecting *ef*, 15,83 inches long; and a third at the other extremity of *ef*, by which *fg* is connected with it. At *i* also is a hinge, uniting the radius *ig* to the middle of *ef*; and then, since *g* moves in a semicircle *egf*, a line joining *e* and *g* would be perpendicular to *fg*.

The piece *cd* has a cavity in the middle of it, so that, when any substance is applied to the middle of the prism P, it may continue to rest horizontally on its extremities. When *ed* has been so elevated that the yellow rays in the fringe of colours (observable where perfect reflection terminates) are seen through the sights, the point *g*, by means of a vernier which it carries, shows by inspection the length of the sine of refraction sought.

The advantages which this method possesses above the usual mode of examining refractive powers, are greater than they may at first sight appear. The usual practice has been, to form two surfaces of the substance under examination, so inclined to each other that the deviation occasioned by them might be measured. The inclination of these surfaces to each other must also be known; and thence the refractive power might be computed. But, in the method here proposed, it is sufficient to have only one surface, and the result is obtained at once, without computation.

The facility of determining refractive powers, is consequently such as to render this property of bodies a very convenient test in many philosophical inquiries. For discovering the purity of essential oils, such an examination may be of considerable utility, on account of the smallness of the quantity requisite for trial. In oil of cloves, for instance, I have met with a wide difference. The refractive power of genuine oil of cloves, is as high as 1,535; but I have also purchased oil by this name, which did not exceed 1,498, and which had probably been adulterated by some less refractive oil.

For such purposes, the refractive power of opaque substances may often be deserving of inquiry, which could not be learned by any means at present in use. For, in the usual mode, a certain degree of transparency is absolutely necessary; but, for trial by contact, the most perfect opacity does not occasion the least impediment.

Among other instances in which I have taken advantage of this circumstance, I may mention a substance that had been found in one of the islands of the North Pacific Ocean, which, to all outward appearance and by various trials, seemed to be perfect bees-wax, although it is supposed that there are no bees in the

island from which it was brought. On placing it by the side of a piece of bees-wax, in contact with a prism, the perfect equality of their refractive powers afforded a strong confirmation of the opinion before formed of their identity.

For the examination also of media of which the refractive density is not uniform, the general method of trial by deviation wholly fails; on the contrary, by placing a varied medium in contact with a prism, all its gradations of density, from greatest to least, become at once the object of mere inspection. An instance of this may very readily be seen with a piece of gum, the surface of which has been moistened for a few minutes; when, by close application to a prism, a refractive power may be discerned, varying from that of the water on the surface, 1,336, to nearly 1,51, the refractive power of gum arabic.

I should not so much insist on this advantage, were it not for the opportunity hereby afforded of examining the crystalline lens of the eye, which is now known to be generally more dense in the centre than at its surface.

Mr. Hauksbee, who was not aware of this difference, has estimated the refractive power of the crystalline lens, by forming it into a wedge by plates of glass, somewhat higher than I find it to be; but, with his accustomed accuracy, he remarked the apparent enlargement of an object, occasioned by the variations of its density, which he was unable to explain.

In the table that follows, I have set down, not only the limits of refractive power in a crystalline lens of an ox, ascertained by trial, but also an average, computed from the refractive density of a dried crystalline of an ox, of which the weight had been first taken in the recent state, and the quantity of water lost by drying also measured.

The table exhibits a series of substances, arranged according to their refractive powers. That of the diamond is copied from Sir ISAAC NEWTON; of other bodies to which (on account of their being more dense than glass) the machine for measurement would not apply, the refractive powers have been found by other means, for the sake of furnishing a more continued series of subjects for comparative experiments. The rest have been compared by this method; and their power, when expressed in numbers, actually measured.

TABLE I.

Diamond	2,44	Horn	—
Plumbago	——	Phosphorus	1,579
Native sulphur (double)	2,04	Mica	—
Glass, consisting of lead 6 and sand 1	1,987	Opium	—
Glass of antimony	1,98	Amber	1,547
Jargon	1,95	Rock crystal (double)	1,547
Spinelle ruby	1,812	*Old plate glass*	1,545
Arsenic	1,811	Colophony	1,543
Muriate of antimony,	variable	Box-wood	—
White sapphire	1,768	Bees-wax	1,542
Gum dragon	—	Oil of sassafras	1,536
Iceland spar, strongest	1,657	Red sealing-wax	—
Sulphate of barytes (double)	1,646	Spermaceti, cold	—
Balsam of Tolu	1,60	Sugar, after fusion	—
Guaiacum	1,596	Arseniate of potash	—
Benzoin	—	Mastic	—
Flint glass	1,586	Elemi	—
Ditto	1,583	White wax (cold)	—
		Oil of cloves	1,535
		Copal	1,535

Anime - -	1,535
Radcliffe crown glass -	1,533
Pitch	—
Centre of crystalline of fish, and dry crystalline of an ox -	1,530
Canada balsam -	1,528
Crown glass, common	1,525
Selenite - -	1,525
Caoutchouc -	1,524
Gum lac	—
Dutch plate glass -	1,517
Human cuticle	—
Gum arabic - -	1,514
Balsam of capivi -	1,507
Oil of amber -	1,505
English plate glass -	1,504
French plate glass -	1,500
Oil of nutmeg -	1,497
Sulphate of potash -	1,495
Tallow, cold - -	1,49
Iceland spar, weakest	1,488
Camphor - -	1,487
Linseed oil - -	1,485
Butter, cold - -	1,480
Essence of lemon -	1,476
Oil of turpentine, common - -	1,476
—— rectified	1,470
Oil of almonds	—
—— olives - -	1,469
—— peppermint -	1,468
—— lavender -	1,467
Tallow, *melted* -	1,460
Alum - - -	1,457
Spermaceti, *melted* -	1,446
Crystalline lens of an ox	1,447
to -	1,380
Computed average of ditto - -	1,430
Sulphuric acid -	1,435
Fluor spar - -	1,433
Nitric acid (sp. gr. 1,48)	1,410
Alcohol - -	1,37
White of an egg -	1,36
Æther - -	1,358
Vitreous humour of an eye - -	1,336
Water - -	1,336
Atmospheric air (HAUKSBEE) -	1,00082

ON THE DISPERSION OF LIGHT.

The method above described for investigating refractive powers, may also be employed with similar advantage for inquiries into the dispersion of light by different bodies, and the consequences that result from their combined action.

When a glass prism is placed in contact with water, and brought near the eye, in such a position that it reflects the light from a window, the extent of perfect reflection is seen to be bounded by a fringe of the prismatic colours, in the order of their refrangibility.* The violet rays, being in this case the most refrangible, appear strongest and lowest, on account of the less obliquity that is requisite for their reflection.

But it may happen that two media, which refract unequally at the same incidence, may disperse equally at that incidence. Under these circumstances, a pencil of rays passing from one of such media into the other, will be refracted, without dispersion of its colours. The boundary of prismatic reflection would then be found a well defined line, free from colour, if the surface at which the reflected light emerges from the prism were at right angles to its course.

When the disparity of the dispersive powers of the media is still greater, it may also happen, that the usual order of prismatic colours will be reversed; and then the red will appear strongest and lowest in the fringe, unless the colours so produced are counteracted by refraction at their emergence from the prism.

An instance in which the colours are so reversed, may be seen by application of oil of sassafras to a prism of flint glass.

* NEWTON'S Optics. Book i. part 2. Exp. 16.

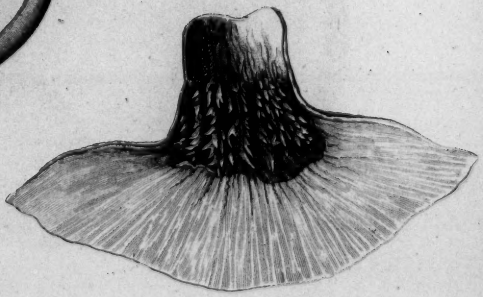
W. Clift del.

F. Bauer sc.

Fig. 1.

Fig. 2.

Fig. 3.

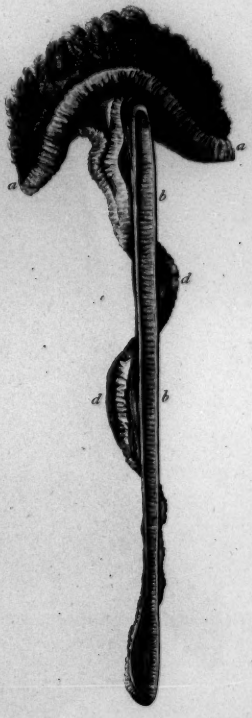


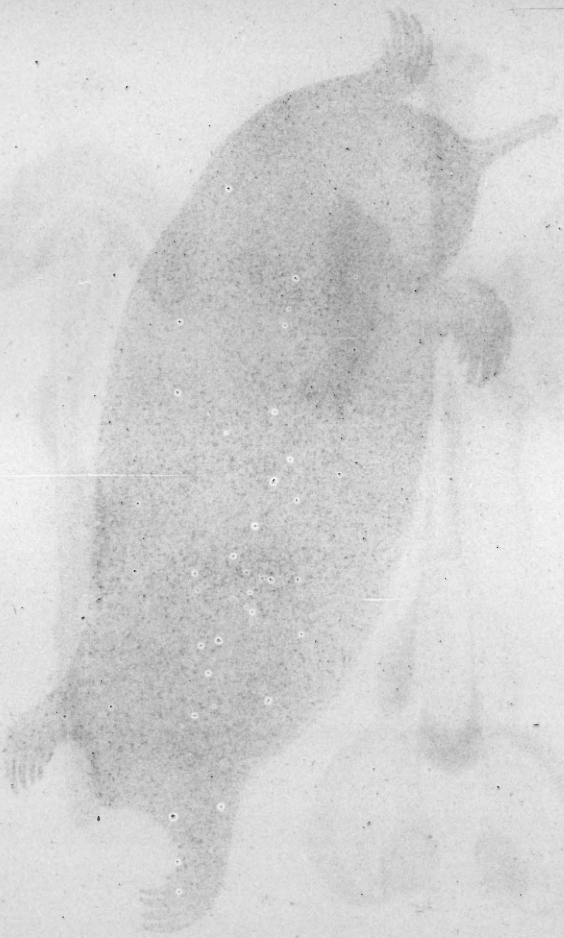
W. Clift del.

F. Basire sc.

Fig. 1.

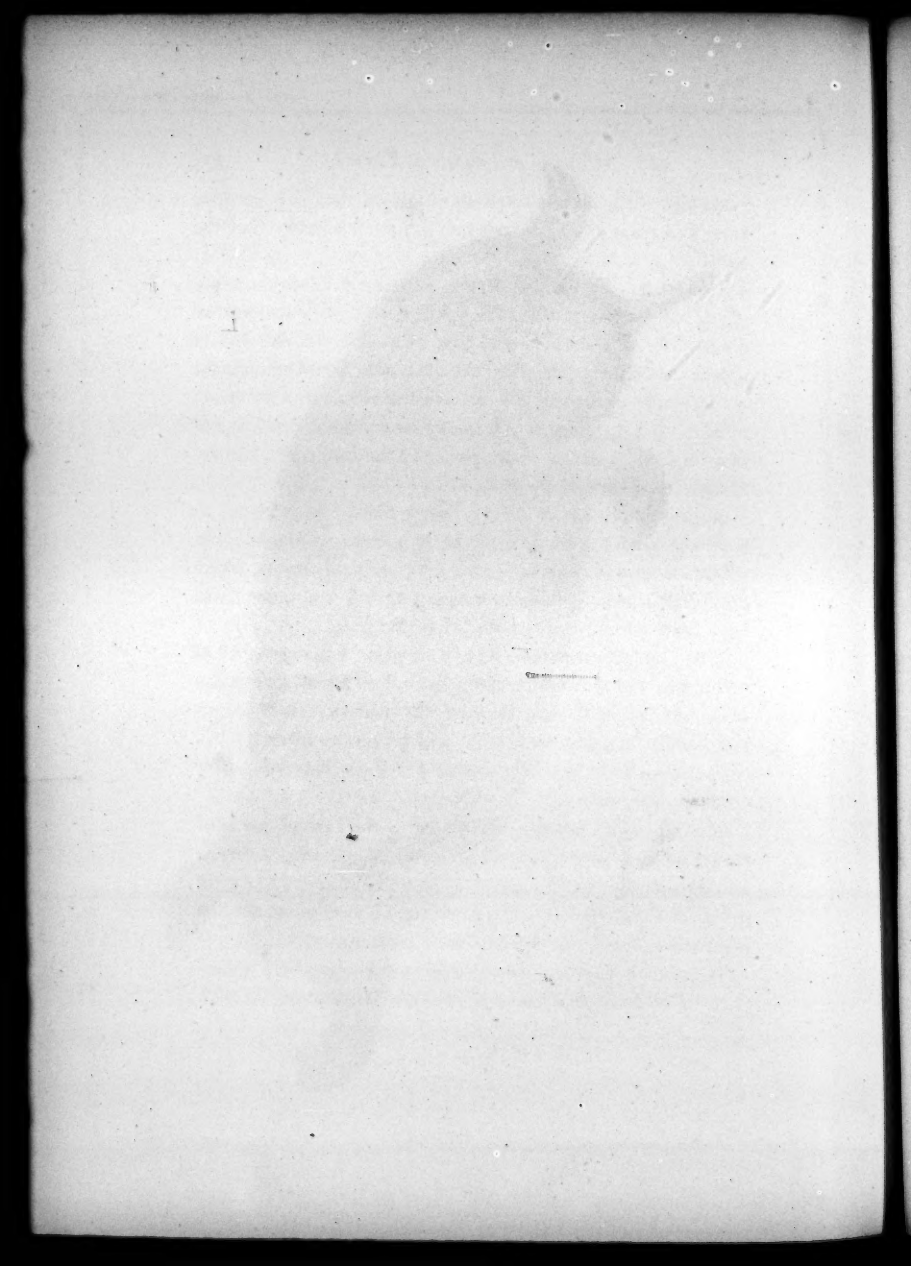
Fig. 2.





W. Clift del.

P. Bassire sc.



So high is the dispersive power of this oil, that, in refractions from flint glass into it, the red rays are refracted more than the violet.

It must be observed that, in this experiment, when the angle of reflection within a triangular prism exceeds 60°, the angle of emergence is such as would alone occasion the red rays to appear lowermost; but, when the glass used is rectangular, the refraction at emergence has an opposite effect; any reversion of colour will therefore be in some degree corrected, and may not be seen, unless the dispersive power of the medium in contact much exceeds that of the glass.

A case of refraction with an inverted order of colours, has been observed by Dr. BLAIR,* in a compound object-glass, where crown-glass was in contact with oil of turpentine. From trials with lenses, he likewise inferred, that several other fluids have the same effect, when applied to that glass.

With this glass, and also with plate-glass, I have tried oil of turpentine, and many other fluids that afford a similar reversion of colours, as linseed-oil, olive-oil, the essential oils of bergamot, lemon, lavender, pennyroyal, and peppermint, strong nitric acid, and many artificial compounds that I shall presently have occasion to mention.

The dispersive power of fluor spar is the least of any substance yet examined; so that, although its refractive power is also remarkably low, (considering its great specific gravity,) a prism of fluor, in contact with water or alcohol, shows the prismatic colours to be refracted in an inverted order.

With heavy spar, the instances of reversion are very numerous, as its dispersive power is low, and is accompanied with

* Edinb. Trans. Vol. III.

great refractive density. In the refractions from this spar into flint glass, and into all oils or resins, I believe, without exception, the colours are seen reversed.

Rock crystal likewise disperses so little, that it exhibits the colours reversed, when it is in contact with many substances of less refractive power than itself. I have tried it with Dutch plate-glass, with Canada balsam and balsam of capivi, with many oils essential and expressed, and have found the colours in all these cases reversed.

By solutions of metallic salts, a great variety of such appearances may be produced. Most of these compounds have a highly dispersive power; and many of them may be rendered sufficiently dense to occasion reversion, even when applied to flint-glass. In a more dilute state, they may be used with crown-glass, or plate-glass, to produce the same effect. And since, when further diluted by a less dispersive medium, they will also present an appearance of colourless refraction, we may, by examining the degree of dilution necessary for that purpose, compare the dispersive powers of any ingredients contained in them, and may gradually extend our knowledge of this property to the elements of any bodies, however compounded.

As a specimen of the method, I have in this way compared a few solutions of metals, and of other substances, that were each diluted till the limit of reflection appeared void of colour, when they were in contact with a rectangular piece of plate-glass; and, in the table which follows, I have expressed their refractive powers in that state of dilution, as nearly as the eye can discern the disappearance of colour.

TABLE II.

	In Water.	In Alcohol.
Nitro-muriate of gold	1,864	1,890
Nitro-muriate of platina	1,870	
Nitrate of iron	1,875	
Sulphuret of potash	1,875	
Red muriate of iron	1,885	
Nitrate of magnesia		
Nitric acid	1,895	
Nitrate of jargon		
Balsam of Tolu	—	1,400
Acetite of litharge (extract of lead)	1,400	
Nitrate of silver		
Nitrate of copper		
Oil of sassafras	—	1,405
Muriate of antimony	—	1,410
Nitrate of lime	1,410	1,422
Nitrate of zinc		
Green muriate of iron	1,415	
Muriate of magnesia	1,416	
——— of lime	1,425	1,440
——— of zinc	1,425	
Essence of lemon	—	1,480
Balsam of capivi	—	1,440

It may here be seen, that several of the metals increase the dispersive powers of nitric and muriatic acids, and consequently exceed them in that respect. Of all these substances that I have yet tried, gold and platina are the most dispersive. The least dispersive of the metals is zinc.

The earths also are found to possess this property in very different degrees: that of the jargon and magnesia differ but little from nitric acid in dispersive power; but siliceous earth, on the contrary, is inferior to water.

By comparing the salts formed with the nitric and muriatic acids, it appeared probable that the former had the higher dispersive power; but a more direct comparison could not be made by means of the rectangular piece of plate-glass, as muriatic acid could not be rendered sufficiently dense for such a trial; I therefore made use of a triangular prism of crown-glass, which is in itself less dispersive than any plate-glass, and, from the relative position of its surfaces, occasioned less correction of the colours. With this prism, I found that strong muriatic acid (having a refractive power 1,394) exhibited the colours reversed; and that, when it was diluted till the limit of reflection appeared void of colour, its refractive power was reduced to 1,382. But the dispersive power of nitric acid, when tried by the same prism, proved to be greater; for this acid required to be diluted till its refractive power did not exceed 1,375, before the colour was wholly destroyed.

In the table it may be observed, that the red and green muriates of iron, though consisting of the same metal and acid, differ very much in dispersive power; and, consequently, that some caution will be necessary, in attempting to compare the different metals with each other by means of the salts containing them, as any difference observed may be owing in part to a difference in the quantity of acid to which they are united, and in part to their different proportion of oxygen.

A striking instance of the latter is manifest, from a comparison of sulphur with the sulphuric acid; for, while the former

appears to exceed the metallic oxides in dispersive power, the latter is inferior even to water.

As I have likewise, at various times, made many experiments on dispersion by means of wedges, in a manner nearly similar to that employed by Mr. DOLLOND, Dr. BLAIR, and others, I have endeavoured to reduce the several substances thus examined to one table; but, as the limits of colour are in few instances sufficiently well defined for accurate mensuration, I have not attempted to add any numerical estimate of their powers, but have merely ascertained the order in which they succeed each other; and, in the following table, have arranged them according to the excess of their effect on violet above red light, at a given angle of deviation.

TABLE III.

Order of dispersive Powers.	Refr. Power.	Order of dispersive Powers.	Refr. Power.
Sulphur	2,04	Amber	1,547
Glass of lead ($\frac{1}{7}$ sand)	1,987	Diamond	2,44
Balsam of Tolu	1,60	Alum	1,457
Oil of sassafras	1,536	Plate-glass, Dutch	1,517
Muriate of antimony		Ditto, English	1,504
Guaiacum	1,596	Crown glass	1,533
Oil of cloves	1,535	Ruby (spinelle)	1,812
Flint-glass	1,586	Water	1,336
Colophony	1,543	Sulphuric acid	1,435
Canada balsam	1,528	Alcohol	1,37
Oil of amber	1,505	Sulphate of barytes	1,646
Jargon	1,95	Selenite	1,525
Oil of turpentine	1,47	Rock crystal	1,547
Copal	1,535	Sulphate of potash	1,495
Balsam of capivi	1,507	White sapphire	1,768
Anime	1,535	Fluor spar	1,433
Iceland spar	1,657		

By comparison of this table with the order of refractive powers, as contained in the first table, it will be seen how little correspondence there is between them; and, accordingly, how numerous are the combinations by means of which a pencil of rays that passes through two media, may be made to deviate without dispersion of its colours.

I cannot conclude these observations on dispersion, without remarking that the colours into which a beam of white light is separable by refraction, appear to me to be neither 7, as they usually are seen in the rainbow, nor reducible by any means (that I can find) to 3, as some persons have conceived; but that, by employing a very narrow pencil of light, 4 primary divisions of the prismatic spectrum may be seen, with a degree of distinctness that, I believe, has not been described nor observed before.

If a beam of day-light be admitted into a dark room by a crevice $\frac{1}{20}$ of an inch broad, and received by the eye at the distance of 10 or 12 feet, through a prism of flint-glass, *free from veins*, held near the eye, the beam is seen to be separated into the four following colours only, red, yellowish green, blue, and violet; in the proportions represented in Fig. 3.

The line A that bounds the red side of the spectrum is somewhat confused, which seems in part owing to want of power in the eye to converge red light. The line B, between red and green, in a certain position of the prism, is perfectly distinct; so also are D and E, the two limits of violet. But C, the limit of green and blue, is not so clearly marked as the rest; and there are also, on each side of this limit, other distinct dark lines, *f* and *g*, either of which, in an imperfect experiment, might be mistaken for the boundary of these colours.

The position of the prism in which the colours are most clearly divided, is when the incident light makes about equal angles with two of its sides. I then found that the spaces AB, BC, CD, DE, occupied by them, were nearly as the numbers 16, 23, 36, 25.

Since the proportions of these colours to each other have been supposed by Dr. BLAIR to vary according to the medium by which they are produced, I have compared with this appearance, the coloured images caused by prismatic vessels containing substances supposed by him to differ most in this respect, such as strong but colourless nitric acid, rectified oil of turpentine, very pale oil of sassafras, and Canada balsam, also nearly colourless. With each of these, I have found the same arrangement of these 4 colours, and, in similar positions of the prisms, as nearly as I could judge, the same proportions of them.

But, when the inclination of any prism is altered so as to increase the dispersion of the colours, the proportions of them to each other are then also changed, so that the spaces AC and CE, instead of being as before 39 and 61, may be found altered as far as 42 and 58.*

* Although what I have above described comprises the whole of the prismatic spectrum that can be rendered visible, there also pass on each side of it other rays, which the eye is not sensible. From Dr. HERSCHEL's experiments (Phil. Trans. for 1800) we learn, that on one side there are invisible rays occasioning heat, that are less refrangible than red light; and on the other I have myself observed, (and the same remark has been made by Mr. RITTER,) that there are likewise invisible rays of another kind, that are more refracted than the violet. It is by their chemical effects alone that the existence of these can be discovered; and, by far the most delicate test of their presence is the white muriate of silver.

To SCHEELE, among many valuable discoveries, we are indebted for having first duly distinguished between radiant heat and light; (*Traité de l'Air et du Feu*, § 56, 57:) and to him also we owe the observation, that when muriate of silver is exposed

By candle-light, a different set of appearances may be distinguished. When a very narrow line of the blue light at the lower part of the flame is examined alone, in the same manner, through a prism, the spectrum, instead of appearing a series of lights of different hues contiguous, may be seen divided into 5 images, at a distance from each other. The 1st is broad red, terminated by a bright line of yellow; the 2d and 3d are both green; the 4th and 5th are blue, the last of which appears to correspond with the division of blue and violet in the solar spectrum, or the line D of Fig. 3.

When the object viewed is a blue line of electric light, I have found the spectrum to be also separated into several images; but the phenomena are somewhat different from the preceding. It is, however, needless to describe minutely, appearances which vary according to the brilliancy of the light, and which I cannot undertake to explain.

to the common prismatic spectrum, it is blackened more in the violet than in any other kind of light. (§ 66.) In repeating this experiment, I found that the blackness extended not only through the space occupied by the violet, but to an equal degree, and to about an equal distance, beyond the visible spectrum; and that, by narrowing the pencil of light received on the prism, the discoloration may be made to fall almost entirely beyond the violet.

It would appear therefore, that this and other effects usually attributed to light, are not in fact owing to any of the rays usually perceived, but to invisible rays that accompany them; and that, if we include two kinds that are invisible, we may distinguish, upon the whole, six species of rays into which a sun-beam is divisible by refraction.

Fig. 1.

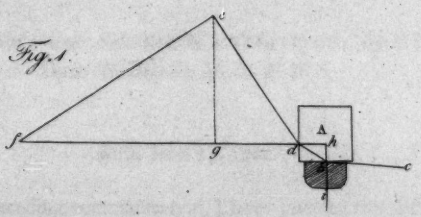


Fig. 2.

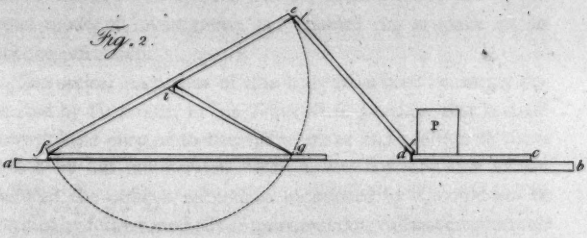
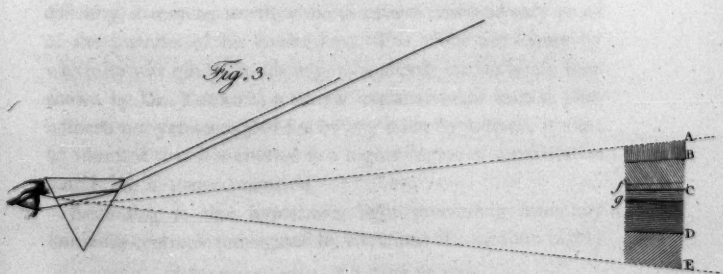


Fig. 3.



Basire Sc.

XIII. *On the oblique Refraction of Iceland Crystal.* By William Hyde Wollaston, *M. D. F. R. S.*

Read June 24, 1802.

In the preceding communication, I have inserted two different measures of refractive powers, distinctly observable in the Iceland crystal, as well as an estimate of its dispersive power; but have reserved for a separate treatise, some remarks which the same mode of investigation has enabled me to make on its oblique refraction.

The optical properties of this body have been so amply described by Huygens, in his *Traité de la Lumière*, that it could answer little purpose to attempt to make any addition to those which he has enumerated. But, as the law to which he has reduced the oblique refractions occasioned by it, could not be verified by former methods of measurement, without considerable difficulty, it may be worth while to offer a new and easy proof of the justness of his conclusions. For, since the theory by which he was guided in his inquiries, affords (as has lately been shown by Dr. Young*) a simple explanation of several phenomena not yet accounted for by any other hypothesis, it must be admitted that it is entitled to a higher degree of consideration than it has in general received.

According to that hypothesis, light proceeding from any luminous centre, is propagated by vibrations of a medium highly

* Bakerian Lecture. Phil. Trans. for 1801.

elastic, that pervades all space. In ordinary cases, the incipient undulations are of a spherical form; but, in the Iceland crystal, light appeared to HUYGENS to proceed as if the undulations were portions of an oblate spheroid, of which the axis is parallel to the short diagonal of an equilateral piece of the crystal, and its centre the point of incidence of the ray.

From this spheroidical form of the undulations, he deduces the obliquity of refraction; and lays down a law, observable in all refractions, at any surface of the spar, whether natural or artificial, which bears the closest analogy to that which obtains universally at other refracting surfaces; for as, in other cases, the ratio is given between the sine of incidence and sine of refraction, (or ordinate of the *spherical* undulation propagated,) so, in the Iceland crystal, the ratio between the sine of incidence and ordinate of refraction (in any one section of the *spheroidical* undulation) is a given ratio.

If ABD (Fig. 1, Plate XV.) be any surface of the spar, let FHOK be a section of the spheroid through its centre C, and RC any ray of light falling on that surface; draw FO a diameter of the spheroid, in the plane of incidence RVO, and CT, its semiconjugate diameter, in the plane of refraction FTO. Then, if CI be the refracted ray, VR, the sine of incidence, shall be to EI, the ordinate of refraction parallel to FC, in the constant ratio of a given line N to the semidiameter FC.

In any other plane of incidence, the ratio of sine to ordinate is also constant; but it is a different ratio, according to the magnitude of that diameter in which the plane of incidence intersects the ellipse FHOK.

When the incidence of a ray passing from any medium of greater density upon a surface of this spar, is such that the

emergent ray becomes parallel to the surface, the ordinate of refraction is then a semidiameter of the spheroid; and, accordingly, the refractive power of this spar, when examined by means of a prism in different directions, should be found to vary as that semidiameter which coincides with the plane of incidence and refracting surface.

The observations that I have made on this substance, accord throughout with this hypothesis of HUYGENS; the measures that I have taken, correspond more nearly than could well happen to a false theory, and are the more to be depended on, as all my experiments, excepting the last, were made prior to my acquaintance with the theory, and their agreement was deduced by subsequent computation.

Exp. 1. The oblique refraction of this spar is rendered visible, by cementing a surface of it to a prism of flint-glass, with a little balsam of Tolu. When the line of sight bisects an acute angle of a natural surface of the spar, the refractive power is seen to be less than in any other direction, and may be expressed by the sine 1,488, or its reciprocal 0,67204.

Exp. 2. When the plane of incidence is parallel to one of the sides, the power is 1,518, of which the reciprocal is 0,6587.

Exp. 3. In a direction at right angles with either side, it is found still higher, being 1,537, or its reciprocal 0,6506.

Exp. 4. And, in the plane bisecting an obtuse angle, the refractive power of the natural surface appears greatest, and is expressed by the sine 1,571, or its reciprocal 0,6365.

Exp. 5. When either of the two greatest solid angles of the spar contained under three obtuse angles, is cut off by a polished surface making equal angles with each of its sides, the same refractive power 1,488 is found in all directions. By the

theory also, the section of the spheroid is in this case a circle, and every semidiameter (FC) the same, since the plane is at right angles to the minor axis.

Exp. 6. If a plane surface be formed bisecting an obtuse angle of the spar, and applied to a prism, the same minimum of refraction 1,488, is found in a direction that coincides with the preceding plane, and therefore with the major axis of the generating ellipse; but, as the direction is varied, it increases so rapidly as soon to exceed the power of glass, and to be no longer ascertainable by the angle of incipient reflection.

Exp. 7. The regular refraction of this spar is also too great for examination by means of any prism, for want of a medium of union of sufficient density; but, by trial in the usual method, it measured, on an average of several experiments, 1,657, or its reciprocal 0,6035.

By assuming, as HUYGENS has done, the equality of this power with the maximum of the oblique refraction, we have sufficient data for construction of the spheroid by which the refractions are regulated; for we have 0,67204 (*Exp.* 1.) as major axis of the generating ellipse, and 0,6035 (*Exp.* 7.) will be the minor axis, parallel in position to the short axis of the spar.

The angle of inclination of this axis to the surfaces of the spar, if supposed to be equilateral, may be computed by spherical trigonometry, from any other angle that has been ascertained by measurement.

The measures that I have taken are not exactly those of HUYGENS; but I nevertheless hold them in equal estimation, from the conformity which I find they bear to each other, by assistance of his theory.

Exp. 8. I measured with care, an angle at which two surfaces of the spar are inclined to each other, and found it to be 105° 5′. Hence, the greater angle of the surfaces themselves may be computed to be 101° 55′; and the angle which the short axis makes with each plane surface is 45° 29′ 25″.

If GSMP (Fig. 2.) be a plane bisecting an obtuse angle of the spar, the section of the spheroid in that plane passes through the axis CS, and therefore is the generating ellipse. By calculating from the known dimensions of its major axis CP 0,67204, its minor axis CS 0,6035, and the angle GCS = 45° 29′ 25″, CG will be found* to be 0,6365, of which the reciprocal is 1,5736, differing but little from 1,571, as it appeared by measurement. (*Exp.* 4.)

Again, if ABDE (Fig. 4.) be one of the natural surfaces, and PG*p* the ellipse formed by that section of the spheroid, PC being as before 0,67204, and CG 0,6365, the reciprocal of 1,571 found by measurement, (*Exp.* 4.) then the semidiameter CT, parallel to the side AE, which makes an angle TCP 39° 2½′, will be found to be 0,6573, instead of 0,6587, and its reciprocal 1,5215, instead of 1,518. (*Exp.* 2.)

The semidiameter also, in the direction of CL, perpendicular to the side, at an angle LCP 50° 57½′, is found by calculation 0,650, and its reciprocal 1,539, instead of 0,6506 and 1,537. (*Exp.* 3.)

From the foregoing data, the course of a ray perpendicular to the surface of the spar may likewise be computed; for, since the sine of incidence is then nothing, the ordinate of refraction must be also nothing, and the ray will be refracted along the semiconjugate diameter CM. (Fig. 2.)

* (Fig. 3.) CS : CP :: tang. PCG : tang. PC*p*.
sec. PC*p* : sec. PCG :: CP : CG.

By calculation,* the angle which this conjugate makes with the perpendicular is $6° 7\frac{1}{2}'$. But, by the following measurement, it appears to be $6° 16'$.

Exp. 9. A piece of spar that measured 1,145 inch in thickness, was laid upon a line, and showed two images that were removed from each other $\frac{126}{1000}$ of an inch. Then, as 1,145 : 0,126 :: radius : tang. of $6° 16'$.

The different results deduced from theory and from observation, will be seen at one view in the following statement.

In *Exp.* 2d,	observed	1,518 ;	calculated	1,5215.
3d,	———	1,537	———	1,539.
4th,	———	1,571	———	1,5736.
9th, angle observed		$6° 16'$	- -	$6° 7\frac{1}{2}'$.

The angle observed differs from that obtained by computation, in a greater degree than any of the former measures; but, when the difficulty of measuring this angle with accuracy is considered, and also the greater effect of any incorrectness in the data from which a semiconjugate is computed, I think the result of this, as well of the preceding comparisons, must be admitted to be highly favourable to the HUYGENIAN theory; and, although the existence of two refractions at the same time, in the same substance, be not well accounted for, and still less their interchange with each other, when a ray of light is made to pass through a second piece of spar situated transversely to the first, yet the oblique refraction, when considered alone, seems nearly as well explained as any other optical phenomenon.

* (Fig. 5.) CS : CP :: tang. PCG : tang. $\frac{1}{2}$CO or co-tang. PCQ;
then CP : CS :: tang. PCQ : tang. PCM;
and LCP — PCM = MCL.

Fig. 1.

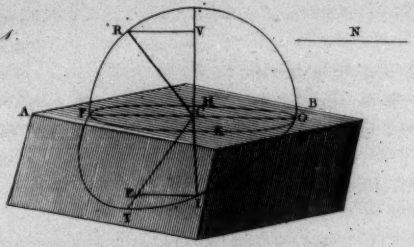


Fig. 2.

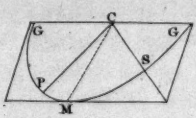


Fig. 3.

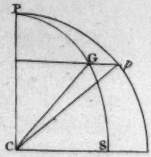


Fig. 4.

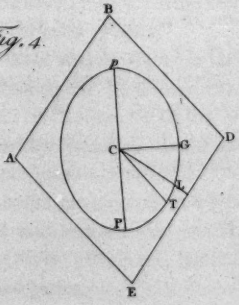
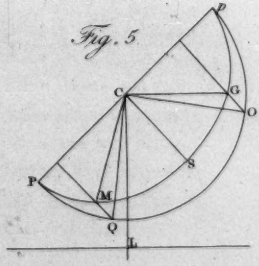


Fig. 5.



XIV. *An Account of some Cases of the Production of Colours, not hitherto described. By* Thomas Young, *M. D. F. R. S. F. L. S. Professor of Natural Philosophy in the Royal Institution.*

Read July 1, 1802.

WHATEVER opinion may be entertained of the theory of light and colours which I have lately had the honour of submitting to the Royal Society, it must at any rate be allowed that it has given birth to the discovery of a simple and general law, capable of explaining a number of the phenomena of coloured light, which, without this law, would remain insulated and unintelligible. The law is, that "wherever two portions of the same "light arrive at the eye by different routes, either exactly or "very nearly in the same direction, the light becomes most "intense when the difference of the routes is any multiple of a "certain length, and least intense in the intermediate state of "the interfering portions; and this length is different for light "of different colours."

I have already shown in detail, the sufficiency of this law for explaining all the phenomena described in the second and third books of NEWTON's Optics, as well as some others not mentioned by NEWTON. But it is still more satisfactory to observe its conformity to other facts, which constitute new and distinct classes of phenomena, and which could scarcely have agreed so well with any anterior law, if that law had been erroneous or imaginary: these are, the colours of fibres, and the colours of mixed plates.

As I was observing the appearance of the fine parallel lines of light which are seen upon the margin of an object held near the eye, so as to intercept the greater part of the light of a distant luminous object, and which are produced by the fringes caused by the inflection of light already known, I observed that they were sometimes accompanied by coloured fringes, much broader and more distinct; and I soon found, that these broader fringes were occasioned by the accidental interposition of a hair. In order to make them more distinct, I employed a horse-hair; but they were then no longer visible. With a fibre of wool, on the contrary, they became very large and conspicuous: and, with a single silk-worm's thread, their magnitude was so much increased, that two or three of them seemed to occupy the whole field of view. They appeared to extend on each side of the candle, in the same order as the colours of thin plates, seen by transmitted light. It occurred to me, that their cause must be sought in the interference of two portions of light, one reflected from the fibre, the other bending round its opposite side, and at last coinciding nearly in direction with the former portion; that, accordingly as both portions deviated more from a rectilinear direction, the difference of the length of their paths would become gradually greater and greater, and would consequently produce the appearances of colour usual in such cases; that, supposing them to be inflected at right angles, the difference would amount nearly to the diameter of the fibre, and that this difference must consequently be smaller as the fibre became smaller; and, the number of fringes in a right angle becoming smaller, that their angular distances would consequently become greater, and the whole appearance would be dilated. It was easy to calculate, that for the light least inflected,

the difference of the paths would be to the diameter of the fibre, very nearly as the deviation of the ray, at any point, from the rectilinear direction, to its distance from the fibre.

I therefore made a rectangular hole in a card, and bent its ends so as to support a hair parallel to the sides of the hole: then, upon applying the eye near the hole, the hair of course appeared dilated by indistinct vision into a surface, of which the breadth was determined by the distance of the hair and the magnitude of the hole, independently of the temporary aperture of the pupil. When the hair approached so near to the direction of the margin of a candle that the inflected light was sufficiently copious to produce a sensible effect, the fringes began to appear; and it was easy to estimate the proportion of their breadth to the apparent breadth of the hair, across the image of which they extended. I found that six of the brightest red fringes, nearly at equal distances, occupied the whole of that image. The breadth of the aperture was $\frac{66}{1000}$, and its distance from the hair $\frac{8}{10}$ of an inch: the diameter of the hair was less than $\frac{1}{500}$ of an inch; as nearly as I could ascertain, it was $\frac{1}{600}$. Hence, we have $\frac{11}{1000}$ for the deviation of the first red fringe at the distance $\frac{8}{10}$; and, as $\frac{8}{10} : \frac{11}{1000} :: \frac{1}{600} : \frac{11}{4800000}$, or $\frac{1}{436}$ for the difference of the routes of the red light where it was most intense. The measure deduced from Newton's experiments is $\frac{1}{392}$. I thought this coincidence, with only an error of one-ninth of so minute a quantity, sufficiently perfect to warrant completely the explanation of the phenomenon, and even to render a repetition of the experiment unnecessary; for there are several circumstances which make it difficult to calculate much more precisely what ought to be the result of the measurement.

When a number of fibres of the same kind, for instance, a uniform lock of wool, are held near to the eye, we see an appearance of halos surrounding a distant candle; but their brilliancy, and even their existence, depends on the uniformity of the dimensions of the fibres; and they are larger as the fibres are smaller. It is obvious that they are the immediate consequences of the coincidence of a number of fringes of the same size, which, as the fibres are arranged in all imaginable directions, must necessarily surround the luminous object at equal distances on all sides, and constitute circular fringes.

There can be little doubt that the coloured atmospherical halos are of the same kind: their appearance must depend on the existence of a number of particles of water, of equal dimensions, and in a proper position, with respect to the luminary and to the eye. As there is no natural limit to the magnitude of the spherules of water, we may expect these halos to vary without limit in their diameters; and, accordingly, Mr. JORDAN has observed that their dimensions are exceedingly various, and has remarked that they frequently change during the time of observation.

I first noticed the colours of mixed plates, in looking at a candle through two pieces of plate-glass, with a little moisture between them. I observed an appearance of fringes resembling the common colours of thin plates; and, upon looking for the fringes by reflection, I found that these new fringes were always in the same direction as the other fringes, but many times larger. By examining the glasses with a magnifier, I perceived that wherever these fringes were visible, the moisture was intermixed with portions of air, producing an appearance similar to dew. I then supposed that the origin of the colours was the same as

that of the colours of halos; but, on a more minute examination, I found that the magnitude of the portions of air and water was by no means uniform, and that the explanation was therefore inadmissible. It was, however, easy to find two portions of light sufficient for the production of these fringes; for, the light transmitted through the water, moving in it with a velocity different from that of the light passing through the interstices filled only with air, the two portions would interfere with each other, and produce effects of colour according to the general law. The ratio of the velocities in water and in air, is that of 3 to 4; the fringes ought therefore to appear where the thickness is 6 times as great as that which corresponds to the same colour in the common case of thin plates; and, upon making the experiment with a plane glass and a lens slightly convex, I found the sixth dark circle actually of the same diameter as the first in the new fringes. The colours are also very easily produced, when butter or tallow is substituted for water; and the rings then become smaller, on account of the greater refractive density of the oils: but, when water is added, so as to fill up the interstices of the oil, the rings are very much enlarged; for here the difference only of the velocities in water and in oil is to be considered, and this is much smaller than the difference between air and water. All these circumstances are sufficient to satisfy us with respect to the truth of the explanation; and it is still more confirmed by the effect of inclining the plates to the direction of the light; for then, instead of dilating, like the colours of thin plates, these rings contract: and this is the obvious consequence of an increase of the length of the paths of the light, which now traverses both mediums obliquely; and the effect is every where the same as that of a thicker plate.

It must however be observed, that the colours are not produced in the whole light that is transmitted through the mediums: a small portion only of each pencil, passing through the water contiguous to the edges of the particle, is sufficiently coincident with the light transmitted by the neighbouring portions of air, to produce the necessary interference; and it is easy to show that, on account of the natural concavity of the surface of each portion of the fluid adhering to the two pieces of glass, a considerable portion of the light which is beginning to pass through the water will be dissipated laterally by reflection at its entrance, and that much of the light passing through the air will be scattered by refraction at the second surface. For these reasons, the fringes are seen when the plates are not directly interposed between the eye and the luminous object; and, on account of the absence of foreign light, even more distinctly than when they are in the same right line with that object. And, if we remove the plates to a considerable distance out of this line, the rings are still visible, and become larger than before; for here the actual route of the light passing through the air, is longer than that of the light passing more obliquely through the water, and the difference in the times of passage is lessened. It is however impossible to be quite confident with respect to the causes of these minute variations, without some means of ascertaining accurately the forms of the dissipating surfaces.

In applying the general law of interference to these colours, as well as to those of thin plates already known, I must confess that it is impossible to avoid another supposition, which is a part of the undulatory theory, that is, that the velocity of light is the greater, the rarer the medium; and that there is also a

condition annexed to the explanation of the colours of thin plates, which involves another part of the same theory, that is, that where one of the portions of light has been reflected at the surface of a rarer medium, it must be supposed to be retarded one half of the appropriate interval, for instance, in the central black spot of a soap-bubble, where the actual lengths of the paths very nearly coincide, but the effect is the same as if one of the portions had been so retarded as to destroy the other. From considering the nature of this circumstance, I ventured to predict, that if the two reflections were of the same kind, made at the surfaces of a thin plate, of a density intermediate between the densities of the mediums containing it, the effect would be reversed, and the central spot, instead of black, would become white; and I have now the pleasure of stating, that I have fully verified this prediction, by interposing a drop of oil of sassafras between a prism of flint-glass and a lens of crown glass: the central spot seen by reflected light was white, and surrounded by a dark ring. It was however necessary to use some force, in order to produce a contact sufficiently intimate; and the white spot differed, even at last, in the same degree from perfect whiteness, as the black spot usually does from perfect blackness.

The colours of mixed plates suggested to me an idea which appears to lead to an explanation of the dispersion of colours by refraction, more simple and satisfactory than that which I advanced in the last BAKERIAN lecture. We may suppose that every refractive medium transmits the undulations constituting light in two separate portions, one passing through its ultimate particles, and the other through its pores; and that these portions re-unite continually, after each successive separation, the one having preceded the other by a very minute but constant

interval, depending on the regular arrangement of the particles of a homogeneous medium. Now, if these two portions were always equal, each point of the undulations resulting from their re-union, would always be found half way between the places of the corresponding point in the separate portions; but, supposing the preceding portion to be the smaller, the newly combined undulation will be less advanced than if both had been equal, and the difference of its place will depend, not only on the difference of the length of the two routes, which will be constant for all the undulations, but also on the law and magnitude of those undulations; so that the larger undulations will be somewhat further advanced after each re-union than the smaller ones, and, the same operation recurring at every particle of the medium, the whole progress of the larger undulations will be more rapid than that of the smaller; hence the deviation, in consequence of the retardation of the motion of light in a denser medium, will of course be greater for the smaller than for the larger undulations. Assuming the law of the harmonic curve for the motions of the particles, we might without much difficulty reduce this conjecture to a comparison with experiment; but it would be necessary, in order to warrant our conclusions, to be provided with very accurate measures of the refractive and dispersive powers of various substances, for rays of all descriptions.

Dr. WOLLASTON's very interesting observations would furnish great assistance in this inquiry, when compared with the separation of colours by thin plates. I have repeated his experiments on the spectrum with perfect success, and have made some attempts to procure comparative measures from thin plates; and I have found that, as Sir ISAAC NEWTON has already

observed, the blue and violet light is more dispersed by refraction, than in proportion to the difference of the appropriate dimensions deduced from the phenomena of thin plates. Hence it happens, that when a line of the light proceeding to form an image of the rings of colours of thin plates, is intercepted by a prism, and an actual picture is formed, resembling the scale delineated by NEWTON from theory, for estimating the colours of particles of given dimensions, the oblique spectrums, formed by the different colours of each series, are not straight, but curved, the lateral refraction of the prism separating the violet end more widely than the red. The thickness corresponding to the extreme red, the line of yellow, bright green, bright blue, and extreme violet, I found to be inversely as the numbers 27, 30, 35, 40, and 45, respectively. In consequence of Dr. WOLLASTON'S correction of the description of the prismatic spectrum, compared with these observations, it becomes necessary to modify the supposition that I advanced in the last BAKERIAN lecture, respecting the proportions of the sympathetic fibres of the retina; substituting red, green, and violet, for red, yellow, and blue, and the numbers 7, 6, and 5, for 8, 7, and 6.

The same prismatic analysis of the colours of thin plates, appears to furnish a satisfactory explanation of the subdivision of the light of the lower part of a candle: for, in fact, the light transmitted through every part of a thin plate, is divided in a similar manner into distinct portions, increasing in number with the thickness of the plate, until they become too minute to be visible. At the thickness corresponding to the ninth or tenth portion of red light, the number of portions of different colours is five; and their proportions, as exhibited by refraction, are nearly the same as in the light of a candle, the violet being the

broadest. We have only to suppose each particle of tallow to be, at its first evaporation, of such dimensions as to produce the same effect as the thin plate of air at this point, where it is about $\frac{1}{10000}$ of an inch in thickness, and to reflect, or perhaps rather to transmit, the mixed light produced by the incipient combustion around it, and we shall have a light completely resembling that which Dr. WOLLASTON has observed. There appears to be also a fine line of strong yellow light, separate from the general spectrum, principally derived from the most superficial combustion at the margin of the flame, and increasing in quantity as the flame ascends. Similar circumstances might undoubtedly be found in other cases of the production or modification of light; and experiments upon this subject might tend greatly to establish the NEWTONIAN opinion, that the colours of all natural bodies are similar in their origin to those of thin plates; an opinion which appears to do the highest honour to the sagacity of its author, and indeed to form a very considerable step in our advances towards an acquaintance with the intimate constitution and arrangement of material substances.

I have lately had an opportunity of confirming my former observations on the dispersive powers of the eye. I find that, at the respective distances of 10 and 15 inches, the extreme red and extreme violet rays are similarly refracted, the difference being expressed by a focal length of 30 inches. Now the interval between red and yellow is about one-fourth of the whole spectrum; consequently, a focal length of 120 inches expresses a power equivalent to the dispersion of the red and yellow, and this differs but little from 132, which was the result of the observation already described. I do not know that these experiments are more accurate than the former one; but I have

repeated them several times under different circumstances, and I have no doubt that the dispersion of coloured light in the human eye is nearly such as I have stated it. How it happens to be no greater, I cannot at present undertake to explain.

CORRECTION OF A FORMER PAPER.

In the Philosophical Transactions for 1800,
P. 146, line 12, for 83810, read 84197;
——— line 15, for .0011562, read .0010116.
In Fig. 53, (Plate VII.) the E♭ (Q) is too near D;
and the E♭ (Y) should be above, instead of below it.

XV. *On the Composition of Emery.* By Smithson Tennant, *Esq. F. R. S.*

Read July 1, 1802.

THE substance called emery, which, from its great hardness, has been long used in various manufactures, for grinding and polishing other bodies, has not, it appears, been hitherto correctly analyzed. In books of mineralogy, it is considered as an ore of iron; an opinion probably derived from its great specific gravity, as well as from the iron which it frequently contains. But, where this metal is most abundant, it could not be extracted from it with advantage, and ought rather to be regarded as an impurity, as it does not contribute to produce the peculiar hardness for which this substance is distinguished. In Mr. KIRWAN's mineralogy, he mentions an examination of emery made by Mr. WIEGLEB, from which he inferred that 100 parts consisted of 95,6 of silex, and 4,4 of iron. Mr. KIRWAN, however, justly suspects the correctness of this account, and observes that he had no doubt but some other stone was imposed upon Mr. WIEGLEB for emery.

When powder of emery is boiled in acids, it becomes of a lighter colour, from the loss of part of the iron; after which, it does not seem to undergo any further alteration. As acids produce so little effect on it, I exposed it to a pretty strong red heat, with carbonate of soda, in a crucible of platina. On adding water to the mass contained in the crucible, the greater part of

the emery was found in powder; having only become of a light colour, from the extraction of part of the iron. Though this process was twice repeated with the remaining powder, and in a stronger heat, a great proportion of it remained undissolved.

The alkaline solution, after a red calx of iron had subsided from it, was saturated with acid; and gave a precipitate of a white earth, which I found to be almost purely argillaceous.

The result of these experiments, was so similar to those of Mr. KLAPROTH on diamond spar, as to render it very probable that emery was in reality the same substance, though usually mixed with a larger proportion of iron; and the subsequent experiments appear to confirm this opinion.

In order to obtain a quantity of emery as free from iron as I could, I reduced to a coarse powder, a piece which consisted of different strata, some of which were of much lighter colour than others; and afterwards separated, by a magnet, the particles which were attracted by it. The part which was not attracted by the magnet, I observed to have the usual degree of hardness, (by the scratches which might be made with it on flint.) I then reduced it to a finer powder, in an agate mortar; and, as this was principally done by pressure, and not by grinding, hardly any sensible addition was made to its weight. In the same manner, I found that diamond spar might be powdered to the same degree of fineness, without any material increase of weight from the mortar.

Of the emery powder thus prepared, 20 grains were taken, and heated in the manner before described, with 120 grains of soda, which had been previously deprived of carbonic acid, and boiled to dryness in a silver pan. By nearly the same process

as that used by Mr. KLAPROTH, I obtained about 16,0 grains of argillaceous earth, ,6 of siliceous earth, ,8 or ,9 of iron, and ,6 of a grain remained undissolved. These numbers, reduced to parts of a hundred, are therefore,

Argillaceous earth	80
Silex	3
Iron	4
Undissolved	3
	90.

Mr. KLAPROTH obtained from the Chinese corundum, after separating from it the particles which were attracted by the magnet,

Argillaceous earth	84
Silex	6,5
Iron	7,5
	98.

As this analysis was no doubt conducted with greater care than mine, the loss of weight was less; but the proportion of the ingredients is sufficiently near to show that the substances are essentially the same.

From 25 grains of emery which appeared the most impregnated with iron, and yet retained its usual hardness, I obtained, argillaceous earth 12,5, silex 2, iron 8, and one grain was not dissolved; or, per cent.

Argillaceous earth	50
Silex	8
Iron	32
Undissolved	4
	94.

As such emery can easily be had of uniform quality in large pieces, I procured the powder employed in this experiment, by rubbing two pieces against each other.

From 25 grains of emery, similar in appearance to the preceding, but which had been digested with marine acid previous to the action of the alkali, I had,

		per cent.
Of argillaceous earth - -	16,4	65,6
Siliceous earth - - -	,8	3,2
Iron - - - -	2,	8,
Not dissolved - - -	4,5	18,0
	23,7	94,8.

The hardness of emery, as far as I could judge by its cutting rock crystal and flint, appeared to be equal to that of diamond spar. The latter could not be scratched by the former; but, as emery has not a surface sufficiently polished to render a mark visible, the converse of this could not be tried.

All the emery which is used in England, is said to be brought from the Islands of the Archipelago, and principally from Naxos. In those places, it is probably very abundant; as the price of it in London, which I was told was 8 or 10 shillings the hundred weight, appears little more than sufficient for the charges of carriage. Though I saw a very large quantity in one place, (more than a thousand hundred weight,) I could not find any pieces of a crystallized form; possibly the great proportion of iron usually mixed with it, may prevent its crystallization. The whole consisted of angular blocks incrusted with iron ore, sometimes of an octaedral form, with pyrites, and very often with mica. The latter frequently penetrates the whole substance of the mass, giving it, when broken, a silvery appearance,

if seen in the direction in which the flat surfaces present themselves to the eye. As these substances have no chemical relation to the emery itself, it is remarkable that they should also accompany the diamond spar from China; for Mr. KLAPROTH observes, "that its lateral facets are mostly coated with a "firmly-adhering crust of micaceous scales, of a silvery lustre:" he also mentions, besides felspar, pyrites, and grains of magnetic iron ore.

XVI. *Quelques Remarques sur la Chaleur, et sur l'Action des Corps qui l'interceptent.* *Par* P. Prevost, *Professeur de Philosophie à Genève, &c.* *Communicated by* Thomas Young, *M. D. F. R. S.*

Read July 1, 1802.

Partie I.

§ 1. Le Dr. Herschel, voulant estimer la quantité de lumière transmise par divers corps, a employé un appareil dont il donne la description détaillée.* Au moyen de cet appareil, il apprécie l'éffet d'une même source de chaleur, agissant d'un côté sans obstacle, et de l'autre à travers une lame qui l'arrête en partie. Qu'on se représente un rayon solaire tombant sur deux thermométres pareils, sur l'un directement, et sur l'autre à travers un verre; qu'on écarte soigneusement toutes les causes étrangères qui pourroient influer ici; et l'on aura une idée de cet appareil, construit avec tout le soin et toute la sagacité qu'on a droit d'attendre d'un excellent observateur.

Ce physicien a fait avec cet appareil un grand nombre d'expériences, toutes de même forme. Chacune d'elles offre six observations, pour chacun des deux thermométres. La 1re observation indique le dégré au commencement de l'expérience, et avant que la source de chaleur ait pu agir; les autres indiquent successivement, de minute en minute, les dégrés de la chaleur croissante, jusqu'à la 5me minute, époque où finit

* Phil. Trans. for 1800. p. 446.

l'expérience.* Ces nombreuses expériences ne diffèrent entr'elles, que par la nature du corps dont est formée la lame interceptante, ou par la nature de la source de chaleur qui est employée.

A la fin de chaque expérience, l'auteur en donne le résultat. Pour cet effet, il retranche le dégré initial du dégré final, et, faisant séparément cette soustraction pour chacun des deux thermométres, il se borne à comparer les restes, pour en conclure la transmission.

Voici le détail de la 1re des expériences de ce genre, qui est la 24me de l'ouvrage.

	Au soleil direct.			A travers un verre blanc bleuâtre.
0′	67°	-	-	67°
1	$68\frac{1}{4}$	-	-	$68\frac{1}{8}$
2	$70\frac{1}{8}$	-	-	$69\frac{1}{8}$
3	$71\frac{3}{8}$	-	-	70
4	$72\frac{3}{8}$	-	-	$70\frac{7}{8}$
5	73	-	-	$71\frac{1}{2}$.

Soustrayant donc le dégré initial du final, on a au soleil direct 6° de chaleur acquise; tandis qu'à travers le verre on n'en a que $4\frac{1}{2}$. Le rapport de ce dernier nombre au premier, répresente la transmission par le verre. C'est en millièmes 0,750; dont le complément 0,250 exprime les rayons interceptés.

§ 2. Les expériences multipliées dont je viens de donner sommairement l'idée, ont été faites surement avec toute l'exactitude qui peut leur donner du prix: elles ouvrent un nouveau champ de spéculation, et font espérer des résultats intéressans. Mais celui que l'auteur a eu en vue, je veux dire, l'appréciation

* Une partie de ces expériences n'a duré que trois minutes: je ne citerai pas cette classe d'experiences, c'est pourquoi j'emploie ici une expression générale.

de la faculté interceptante de la lame mise en expérience, n'est pas aussi simple qu'il le paroît au premier coup-d'œil, et exige une nouvelle recherche.

Et d'abord, si de l'expérience que j'ai citée et transcrite ci-dessus, on pouvoit inférer d'une manière générale, que le nombre des rayons interceptés par le verre blanc bleuâtre est exprimé par la fraction 0,250, cela ne devroit pas être particulier au temps de la durée de l'experience. Si, par exemple, elle avoit duré six ou sept minutes, on devroit trouver le même résultat; le même encore, si elle n'avoit duré que trois ou quatre minutes. Nous ne pouvons parler expérimentalement que de ce dernier cas, qui est consigné dans le régistre de l'expérience. Or, il est facile de voir, que si l'auteur s'étoit arrêté à la 4me minute, il auroit trouvé la transmission exprimée par le rapport $3\frac{7}{8} : 5\frac{1}{8} = 0{,}720$, et par conséquent la faculté interceptante = 0,280. S'il se fût arrêté à la 3me minute, l'interception eût été 0,314; a la 2de minute, 0,320; et à la 1re minute, 0,357. Ensorte qu'on devroit croire, en suivant la marche tenue ici, qu'à la 1re minute le verre interceptoit plus de rayons qu'à la 2de; plus à la 2de qu'à la 3me; à la 3me qu'à la 4me; à la 4me qu'à la 5me.

Le même résultat, à une seule irrégularité près,* peut se déduire de l'expérience suivante, (où l'auteur a substitué une lame de *flint glass* à celle de verre blanc bleuâtre de la précédente, et dont il donne le détail,) que je transcrirai ci-dessous. (§ 9.) L'auteur dit lui-même, que ce résultat a été commun à toutes les expériences analogues, à l'exception d'une seule, qui lui a paru anomale à d'autres égards.† Il emploie même ce fait

* A la fin de la 3me minute, la transmission a été excédente.

† P. 479. Exp. 122. Je rapporterai bientôt cette expérience en détail. (§ 11.)

comme un argument, pour prouver que les rayons chauds et lumineux sont différens.* On peut donc envisager ce résultat comme général et constant. Ensorte que, si la mesure de la transmission adoptée ici est juste, on doit croire que la faculté interceptante des corps va toujours en croissant, de minute en minute, au moins jusqu'à la 5me; et, si jamais un raisonnement analogique est admissible, on doit croire que cette progression croissante dureroit après les cinq minutes écoulées; si bien qu'en prolongeant l'expérience, on fait croître la faculté interceptante de la lame, et la chaleur transmise doit à la longue diminuer beaucoup, ou même enfin se réduire à rien.

Cette conséquence, qui est inévitable dans cette méthode d'estimation, doit peut-être inspirer du doute sur le principe dont elle dérive; car on ne sauroit concevoir aucune raison probable, pour laquelle la faculté de transmettre ou d'intercepter la chaleur doive varier dans un corps, parcequ'il y a plus ou moins long-temps qu'il la transmet ou l'intercepte.

Cette difficulté nous ramène à la théorie de la chaleur, et en particulièr de la communication de la chaleur, de son passage d'un lieu dans un autre, ou d'un corps dans un autre corps. En effet on voit ici, de part et d'autre, la boule d'un thermométre plongée dans une source de chaleur, telle qu'un rayon solaire, par exemple; on voit la chaleur passer de la source dans la boule, et amener celle-ci, peu-à-peu, à une température plus haute que celle dont elle jouissoit; on voit qu'il s'écoule plusieurs

* P. 522. " L'Interception de la chaleur solaire," dit-il, " a constamment été plus " grande près du commencement des cinq minutes que vers la fin; or, cela n'a pas " lieu dans la transmission de la lumière, qui est sensiblement instantanée. Cela " indique que la loi de transmission n'est pas la même pour la lumière que pour la " chaleur." Ce sont là ses expressions abrégées, et réduites au seul objet que j'ai en vue.

minutes avant que cet accroissement de température vienne à cesser; et il ne paroît pas qu'on ait atteint le terme de son maximum. Il y auroit donc de l'importance à connoître la loi de cet accroissement; car il est facile de comprendre que, selon la nature de cette loi, l'accroissement partiel produit pendant un temps limité (tel que 5′) sera, ou ne sera pas, proportionnel à la chaleur de la source. Or, c'est cette chaleur qu'il s'agit d'estimer, d'une et d'autre part, pour pouvoir comparer la chaleur transmise par le verre avec la chaleur entière qui passe sans obstacle. Tournons donc notre attention vers un objet si évidemment requis.

§ 3. La loi dont nous avons besoin, a été reconnue et déterminée par des expériences directes. Il résulte de celles de M M. KRAFT et RICHMANN,* que *dans un milieu d'une température constante,* un corps s'échauffe ou se réfroidit de sorte que *les différences de sa chaleur à celle du milieu sont en progression géométrique, tandis que les temps* de l'échauffement ou du réfroidissement *sont en progression arithmétique.* Cette loi, déduite, je le répéte, d'expériences directes et faites avec soin, est parfaitement d'accord avec la théorie générale de la chaleur, qui se fonde sur d'autres faits, et dont je dirai un mot en finissant ce mémoire. En ce moment, je laisse cette loi isolée, et je l'admets simplement comme une vérité particulière, que l'expérience a démontrée.

§ 4. Il résulte de cette loi d'accroissement, que si deux corps de même température sont plongés dans deux milieux de température constante, mais inégale, les accroissemens opérés en temps égaux ne seront point, en général, proportionnels à la température de ces milieux, puisqu'il n'y a que quelques cas

* *Nov. Comm. Acad. Petrop.* Tom. I. p. 195.

très-particuliers et très-rares où cette proportion puisse avoir lieu, comme il est facile de s'en assurer; par conséquent, les deux thermométres des expériences précédentes n'indiquent pas, par le rapport de leurs mouvemens en cinq minutes, le rapport de la chaleur totale à la partie de cette chaleur qui a été transmise par la lame interposée. Il faut s'y prendre d'une autre manière, pour faire cette estimation. La plus simple, peut-être, eût été de n'avoir aucun égard au temps, et de laisser chaque thermométre atteindre la température de la source de chaleur dans laquelle il est plongé; mais la durée de l'expérience entreprise sur ce principe offre peut-être des inconvéniens, et l'on verra d'ailleurs, par ce qui va suivre, que cette méthode même exigeroit encore une analyse ultérieure. Quoiqu'il en soit, on peut encore tirer des conséquences légitimes, des expériences qui ont été faites dans un temps limité. Je vais m'appliquer a tirer ces conséquences, du moins les principales; et l'auteur de cette belle suite d'observations verra, j'espère, avec plaisir, que les resultats qu'elles offrent sous cette nouvelle forme deviennent plus réguliers et plus probables.

§ 5. Je commence par discuter l'expérience 24me de l'ouvrage de M. HERSCHEL; c'est celle dont j'ai transcrit le détail ci-dessus. (§ 1.) Les temps croissant en proportion arithmétique, 0, 1, 2, 3, 4, 5, les différences de chaleur du thermométre et du milieu doivent décroitre en progression géométrique. Les dégrés observés au thermométre exposé à la chaleur libre du soleil sont, au commencement des trois premières minutes, 67, $68\frac{3}{4}$, $70\frac{1}{8}$, ou en huitièmes de dégré, 536, 550, 561. Maintenant, si l'on suppose que la température du rayon solaire ait été (en huitièmes de dégré) = 601, on trouvera que les différences de la chaleur du thermométre à celle du milieu, savoir, 65, 51, 40,

sont en progression géométrique; ce qu'on n'obtiendra par aucun autre nombre. La loi prescrite nous force donc d'admettre ce nombre, pour l'expression de la chaleur du milieu où étoit placé le thermomètre. Cela étant, nous calculerons les termes suivans de la progression, nous en conclurrons les dégrés du thermomètre pour les minutes suivantes, et nous les comparerons aux dégrés observés. C'est l'objet de la petite table suivante, où tous les nombres expriment des huitièmes de dégré.

Chaleur du milieu, conclue des 3 *premiers termes* . . . . 601.

	Dégrés observés.	Dégrés calculés.	Différences en progr. géom.
0′	536	536	65
1	550	550	51
2	561	561	40
3	571	570	31
4	579	576	25
5	584	582	19

On peut observer, que les trois derniers dégrés calculés sont d'accord avec les dégrés observés, avec un écart de moins de trois huitièmes de dégré.

§ 6. Maintenant nous allons faire la même opération pour les observations collatérales, faites avec le thermomètre que garantissoit un peu une lame de verre blanc bleuâtre. Mais il y a ici une remarque à faire: la progression des différences du premier thermomètre et du milieu a pour quotient $\frac{44}{51}$; il paroît que celle du second thermomètre doit avoir le même quotient, car il part du même point, son échelle d'échauffement est comprise en entier dans celle du premier thermomètre, et ces deux thermomètres ont été choisis avec une attention scrupuleuse, de manière à avoir précisément la même sensibilité; ainsi, par un

même accroissement de chaleur, chacun d'eux, en même temps, se meut d'une même quantité. Si, par exemple, la température du milieu excéde, de part et d'autre, celle du thermométre de 65 huitièmes de dégré, on doit s'attendre que l'un et l'autre en une minute en acquerra 14, et ne différera plus de la source que de 51 huitièmes de dégré; mais, en chaque thermométre, cette proportion étant constante dans les échauffemens subséquens, (d'après la loi,) il est clair que le quotient est le même pour les deux thermométres, dans toute l'étendue de la progression.

Il n'en seroit pas ainsi, si les thermométres n'étoient pas également sensibles; par conséquent, en passant d'une expérience à l'autre, il conviendra de remarquer si les thermométres ont changé; et, en ce cas, de chercher de nouveau le quotient de la progression.

§ 7. Je viens à la partie de l'expérience qui nous reste à examiner; il s'agit du thermométre garanti de l'action du soleil, par une lame de verre blanc bleuâtre. Prenant donc les deux premiers nombres donnés par l'observation, savoir, ceux qui repondent au commencement et à la fin de la 1re minute de l'expérience, nous déterminerons celui qui a dû exprimer la chaleur du milieu, pour que les différences des deux premiers nombres à celui-ci soient entr'eux comme 65 est à 51; et, formant successivement les autres termes de cette progression, nous en conclurons les dégrés pour les quatres minutes suivantes, afin de les comparer aux dégrés observés. C'est l'objet de la petite table suivante, en huitièmes de dégré.

Chaleur du milieu 578.

	Dégrés observés.	Dégrés calculés.	Différences en progr. geom.
0′	536	536	42
1	545	545	33
2	553	552	26
3	560	558	20
4	567	562	16
5	572	565	13

Les nombres calculés et observés différent ici de 1 jusqu'à 7 huitièmes de dégré. Je dirai plus bas à quoi j'attribue cet écart. (§. 12.)

§. 8. Supposant maintenant que la chaleur de l'un et de l'autre milieu (celle du rayon libre et celle du courant qui agit sous le verre) ait été bien appréciée, il ne reste plus qu'à les comparer; leur rapport est celui de 601 à 578; et par conséquent la quantité interceptée = 0,038.

§. 9. Passons à l'expérience suivante, qui est la 25me de l'ouvrage. Celle-ci a été faite avec les mêmes thermométres que la précédente. Le corps mis en expérience étoit une lame de *flint glass;* et en voici le résultat, tel que le donne l'auteur.

	Dégrés observés.	
	Au soleil libre.	A travers le *flint glass.*
0′	$69\frac{3}{4}$	$69\frac{3}{4}$
1	$71\frac{1}{4}$	71
2	$72\frac{5}{8}$	$72\frac{1}{8}$
3	$74\frac{1}{8}$	$73\frac{7}{8}$
4	$74\frac{7}{8}$	74
5	$75\frac{1}{4}$	$74\frac{3}{4} \ldots\ldots 5\frac{1}{2} : 5 = 0{,}909$

En calculant cette expérience comme la précédente, et en prenant $\frac{45}{51}$ pour le quotient de la progression des différences, on aura, en huitièmes de dégré, les resultats comparés qu'indique la table suivante.

	Au soleil libre. *Chaleur du milieu* 614.			*A travers le* flint glass. *Chaleur du milieu* 604.		
	Dégrés observés.	Dégrés calculés.	Différ. en progr. geom.	Dégrés observés.	Dégrés calculés.	Différ. en progr. geom.
0′	558	558	56	558	558	46
1	570	570	44	568	568	36
2	581	579	35	577	576	28
3	593	587	27	591	582	22
4	599	593	21	592	587	17
5	602	597	17	598	590	14

Rapport des deux chaleurs 604 : 614.
Interception - - 0,015.

§. 10. En jettant les yeux sur cette table, on voit que les deux thermométres ont montré un échauffement plus rapide dans les dernières minutes que le calcul ne l'annonçoit. Le thermométre au soleil libre, présente un excès de 1 jusqu'à 6 huitièmes de dégré, qui diminue à la fin, et se reduit à 5 huitièmes. Le thermométre garanti par le *flint glass*, présente un excès qui varie irrégulièrement de 1 à 9 huitièmes. Cet écart, pour le thermométre garanti, est dans le même sens que celui de l'expérience précédente, et sera expliqué de même ci-dessous. (§. 12.) L'écart du thermométre exposé au soleil libre, ne peut s'expliquer qu'en supposant quelque cause particulière d'irregularité.

§. 11. Il y a une troisième expérience dont l'observateur donne le detail, et qu'il nous reste à examiner; c'est la 122me de l'ouvrage; elle a été faite avec une lame de talc, sous l'influence de la chaleur d'un feu de charbon bien ménagé. En voici les résultats donnés par l'auteur.

	Dégrés observés.	
	Au feu libre.	A travers le talc.
0′	65	65
1	72	67
2	77	$68\frac{3}{4}$
3	$80\frac{1}{2}$	$69\frac{1}{2}$
4	83	70
5	85	$70\frac{3}{4}$ . . . . . 20 : $5\frac{3}{4}$ = 0,287.

Ici les thermométres ne sont plus les mêmes que ceux qui ont été employés dans les deux expériences que nous avons discutées; ceux-ci étoient désignés No. 5, et No. 1; ceux-là sont distingués par les lettres D, C; il faut donc chercher de nouveau, pour ces deux thermométres, également sensibles entr'eux, (mais peut-être différens en sensibilité des precedens,) selon quelle progression s'est fait l'échauffement au soleil libre, pendant le cours des deux premières minutes, (§ 6,) afin d'en conclure les dégrés suivans. Il résultera de ce calcul, et de l'emploi de la progression ainsi déterminée pour le thermométre garanti, la table suivante, toujours en huitièmes de dégré.

	Au feu libre. *Chaleur du feu 716.*			*A travers le talc.* *Chaleur du milieu 576.*		
	Dégrés observés.	Dégrés calculés.	Différ. en progr. geom.	Dégrés observés.	Dégrés calculés.	Différ. en progr. geom.
0′	520	520	196	520	520	56
1	576	576	140	536	536	40
2	616	616	100	550	547	29
3	644	645	71	556	555	21
4	664	665	51	560	561	15
5	680	680	36	566	566	10

La progression des différences a ici pour quotient $\frac{7}{5}$, au lieu de $\frac{45}{51}$: ainsi les thermométres recevoient, en temps égal, de la source calorifique, une aliquot de chaleur un peu moindre que les précédens; cependant, la différence n'est pas très considérable; du reste, on peut bien dire, que dans cette expérience le calcul et l'observation sont parfaitement d'accord. Ce n'est pas la peine de remarquer des différences aussi petites, et qui seroient encore plus insensibles, si j'avois tenu compte des fractions de dégré inférieures à une huitième, ce que je n'ai pas cru devoir faire. Cet accord est d'autant plus remarquable, que c'est précisément ici l'expérience qui a offert quelque chose de particulier, qui auroit dû, à ce qu'il semble, introduire de l'irrégularité dans les résultats. Le talc s'est calciné par l'action du feu, dans le cours de l'expérience, et de transparent qu'il étoit, il est devenu parfaitement opaque; néanmoins, il paroit que l'action de la chaleur sous le talc, de minute en minute, a suivi un cours parfaitement régulier et uniforme. En voici le calcul.

Rapport des deux chaleurs 576 : 716.

Interception - - 0,196.

§ 12. Tels sont les résultats que nous offrent les trois expériences dont l'auteur a consigné le détail dans son ouvrage. Il

est temps de dire un mot de la cause à laquelle j'attribue, dans les deux premières expériences, l'excès d'échauffement qui a été observé au thermométre garanti, dans les dernières minutes de leur durée. (§§ 7 et 10.) Je crois qu'il depend de la chaleur accumulée dans le corps interceptant. A l'instant où ce corps s'échauffe, il contribue à faire monter le thermométre voisin. Si la marche de cette accumulation de chaleur étoit très régulière, son effet se confondroit avec celui des rayons transmis; (c'est, je pense, ce qui a eu lieu dans la 3me expérience, où la progression des différences n'est guères moins exacte pour le thermométre garanti que pour l'autre;) mais, si l'accumulation est accélérée, (c'est-à-dire, si le rapport des rayons accumulés aux transmis est plus grand en même temps vers la fin de l'expérience qu'au commencement,) son effet croissant se fera sentir au thermométre, qui se mouvra comme il s'est mû dans les deux premières expériences. A quoi donc peut tenir une pareille accélération, et quelle raison peut on imaginer pour qu'elle ait lieu dans un cas, et non dans l'autre? On ne sauroit, je crois, l'imputer à aucune cause plus probable qu'à l'épaisseur de la lame, ou à la foiblesse de la source de chaleur.

§ 13. Supposons qu'on présente un verre épais à un foyer de chaleur; il s'échauffera du côté du feu, et, conduisant mal la chaleur, il restera quelque temps froid du côté opposé; ainsi, pendant la 1re minute, peut-être, un thermométre placé de ce dernier côté n'accuseroit aucun échauffement; mais, peu-à-peu, dans les suivantes, cet échauffement se feroit sentir. Je présume que c'est ainsi que les choses se sont passées dans les deux premières expériences, et en particulier dans la seconde; (la 25me de l'ouvrage;) dans celle-ci, la lame de *flint glass* avoit environ trois lignes d'épaisseur. L'observateur donne cette mesure,

tandis qu'il ne dit rien de l'épaisseur des autres lames. Il est probable que celles-ci étoient plus minces, en particulier celle de talc; et cela pourroit expliquer la régularité de l'une de ces expériences, et l'irregularité de l'autre.

Joignez à cela, que dans la troisième des expériences que j'ai analysées, (la 122me de l'ouvrage), la source de chaleur (le feu de charbon) avoit plus d'intensité, ou d'activité, que celles (les rayons solaires) qui agissoient dans les deux autres; puisque, dans le même espace de cinq minutes, elle a amené le thermomètre libre de 65° à 85; tandis que le thermomètre libre dans les deux autres expériences, n'a monté que de 5 ou 6 dégrés, compris entre ces extrêmes. Or, il est probable, que si deux lames sont de même nature et de même épaisseur, mais que l'une soit exposée à une chaleur forte et l'autre à une chaleur foible, la première sera traversée plutôt que la seconde, par la chaleur accumulée; ensorte que, touchant, à la fin de la 1re minute, par exemple, la face non exposée de chacune des deux lames, il se pourra faire qu'on sente l'une froide et l'autre chaude.

Par deux raisons donc, l'expérience 122me a dû offrir des résultats réguliers; 1. parceque probablement la lame étoit mince; 2. parceque la source de chaleur étoit grande; d'où il résultoit, que la chaleur accumulée l'avoit traversée dès la fin de la 1re minute; ensorte que l'accumulation, et le rayonnement qui en est la suite, croissoient, de minute en minute, selon la même loi d'échauffement selon laquelle s'échauffoit d'ailleurs la boule du thermomètre, si quelque chaleur étoit transmise sans obstacle.

Et si la 2de expérience (la 25me de l'ouvrage) offre plus d'irregularités que la première, (la 24me de l'ouvrage,) cela pourroit bien tenir en partie à la plus grande épaisseur du *flint*

glass. Cependant, d'un côté nous ne pouvons rien affirmer sur l'épaisseur du verre blanc bleuâtre, qui n'est pas indiquée; et de l'autre, l'échauffement au soleil libre offre, dans cette même expérience, (la 25me,) des écarts qui vont jusqu'à $\frac{6}{8}$mes de dégré. Pourroit on les attribuer à quelque légére variation dans la source même de la chaleur, pendant le cours de l'expérience?

Je pense en avoir dit assez, pour rendre probable la cause à laquelle j'attribue cette espèce d'irrégularité apparente, qui consiste dans l'accélération de l'échauffement du thermométre garanti; cette cause doit avoir été, l'inégale action de la chaleur accumulée sur le corps interceptant, au commencement et à la fin de l'expérience.

§ 14. Il résulte de ces considérations, et de la distinction entre les deux chaleurs, transmise et accumulée, que l'interception calculée ci-dessus, dans chacune des trois expériences que nous avons rapportées, n'est, à proprement parler, qu'une limite en dessous, et laisse indéterminée la limite supérieure. Car, comme nous ne savons point le rapport des deux chaleurs, (transmise et accumulée,) nous ne pouvons point affirmer l'influence de chacune d'elles sur le résultat. Si la chaleur librement transmise agissoit seule, nous aurions une progression régulière de différences, (comme on l'a au soleil libre,) et les dégrés calculés s'accorderoient aussi bien avec ceux qu'a donnés l'observation. Mais il y a excès dans les derniers termes; et cet excès doit provenir de la chaleur accumulée; celle-ci a donc agi, et manifesté son influence. D'un autre côté, la transmission libre peut avoir été fort petite; on pourroit même la supposer nulle, et attribuer à la chaleur accumulée, tout l'effet observé sur le thermométre garanti. Ainsi l'on peut bien dire, que la transmission réelle n'a pas été plus grande que la calculée, puisque le calcul suppose

tout l'effet produit par cette chaleur; mais elle peut très bien avoir été moindre, puisque cet effet a certainement été produit, en partie au moins, et peut-être en totalité, par une autre cause. L'interception peut donc avoir été totale, ou très grande, mais jamais moindre que celle que le calcul nous a donnée. C'est en ce sens qu'il faut prendre tous nos résultats obtenus jusqu'ici, et tous ceux que nous allons rechercher encore.

§ 15. Il y auroit maintenant quelque intérêt à examiner, d'après les calculs précédens, combien auroit dû durer chaque expérience, pour que le thermomètre atteignît le maximum d'échauffement, c'est-à-dire, la température de la source, ou du milieu dans lequel il étoit plongé; car c'est à cette époque qu'on auroit pu comparer immédiatement les dégrés des deux thermomètres, exposés, l'un à la chaleur libre, et l'autre à la chaleur gênée par l'interception. Cependant, une difficulté se présente. Il est facile de continuer les termes de la progression au soleil libre, et d'en conclure les dégrés qu'on auroit observés dans les minutes suivantes; mais, pour le thermomètre garanti, comment tenir compte de l'effet inégal de la chaleur accumulée dans la lame interceptante? Arrivée à un certain point, cette chaleur accumulée, n'en développera-t-elle point même de nouvelle, comme il semble que cela a lieu dans les boules d'argile échauffées au feu d'un foyer? Quoiqu'il en soit, comme ceci n'intéresse point l'échauffement au soleil libre, nous pouvons du moins examiner ce cas. J'y joindrai le calcul de l'échauffement sous le talc, à cause de sa régularité, qui semble indiquer que, dans les termes suivans, la progression auroit été constante.

Comme l'observateur tient compte des huitièmes de dégré, et non d'aucune fraction moindre, l'échauffement paroîtra fini plutôt qu'il ne le sera réellement. Ainsi, vers la fin, on ne re-

marquera plus de différence sensible pendant une minute; mais, en attendant deux ou trois minutes, cet accroissement se fera remarquer. Je trouve que dans la 1re expérience, (la 24me de l'ouvrage,) au soleil libre, le thermomètre auroit continué jusqu'à la 12me minute, d'accuser, de minute en minute, un accroissement de chaleur sensible: il auroit alors marqué 598 huitièmes de dégré. Il se seroit passé encore quelques minutes, avant que le thermomètre eût acquis sensiblement (c'est-à-dire, à un huitième près) la chaleur totale de la source, qui, selon notre calcul, (§ 5,) étoit de 601 huitièmes de dégré.

Je laisse l'expérience faite avec le *flint glass*, (la 25me de l'ouvrage,) à cause de son irrégularité.

Celle où le talc a été employé (la 122me de l'ouvrage) nous fait voir, qu'au soleil libre il auroit aussi fallu 12′ pour amener le thermomètre assez près de la température du milieu, pour que l'échauffement en une minute fût devenu insensible; (c'est-à-dire, moindre qu'un huitième de dégré;) à cette époque, il n'auroit différé que d'environ $\frac{1}{8}$mes de la température du milieu, qu'il auroit assez vîte atteint.

Dans cette même expérience, le thermomètre couvert de la lame de talc n'auroit requis que 9′, pour arriver au terme auquel une minute de plus ne produit aucun effet sensible; à cette époque, la chaleur du thermomètre auroit différé de celle du milieu d'un peu moins de $\frac{1}{8}$mes de dégré; et 3 minutes après, c'est-à-dire, à la 12me minute de l'expérience, ces deux chaleurs n'auroient pas différé sensiblement; je veux dire, qu'elles auroient différé d'une quantité moindre qu'un huitième de dégré, qui est la fraction la plus petite dont l'observateur ait tenu compte.

§ 16. Jusqu'ici je n'ai discuté que trois expériences, entre toutes celles du même genre, parceque ce sont les seules dont l'auteur

donne le détail. Pour toutes les autres, il se contente de rapporter le dégré initial et le dégré final de chaque thermométre, parcequ'en effet ce sont les seuls qu'il emploie, pour en conclure, par sa méthode, la quantité des rayons transmis et interceptés. Il sera facile à l'auteur de vérifier ces remarques, par l'examen de ses régistres plus détaillés. Pour suppléer à cette recherche, qui n'est pas en mon pouvoir, j'ai essayé d'employer, d'une manière conforme aux principes exposés ci-dessus, quelques-uns des résultats abrégés, qui s'offrent à nous en grand nombre.

§ 17. On peut remarquer que le rapport de 13 à 10, est moyen entre ceux qui ont été employés comme quotients de la progression des différences, et que l'observation a déterminés. (§§ 5 et 11.) Je me tiendrai donc à ce rapport; et je déterminerai la chaleur constante du milieu par la proportion suivante. Les différences entre cette chaleur et chacun des nombres donnés par l'observation, (l'initial et le final,) sont entr'elles comme le 1er terme de la progression est au 6me, c'est-à-dire, comme les nombres 13 et 10 élévés à la cinquième puissance.

§ 18. Ainsi, prenant la 26me expérience de l'ouvrage, on l'y trouvera ainsi abrégée:

	Au soleil libre.	A travers du *crown glass* verdâtre.
0′	$66\frac{1}{4}$	$66\frac{1}{4}$
5	73	$71\frac{1}{4}$ ..... $6\frac{3}{4} : 5 = 0{,}741$

J'en conclus, (en partant du rapport de 13 à 10 pour la progression des différences,) que la chaleur constante du soleil libre étoit, en huitièmes de dégré, $60\frac{4}{5}$; et à travers le verre $58\frac{4}{5}$.

Rapport de ces chaleurs 0,967

Interception 0,033.

27*me*.

	Soleil.	*Coach glass* (verre de carrosse.)
0′	$68\frac{7}{8}$	$68\frac{7}{8}$
5	$75\frac{7}{8}$	$74\frac{3}{8}$ ..... $7 : 5\frac{1}{2} = 0{,}786$

Chaleur au soleil libre 267
Sous le verre - - - 611
Rapport 0,974
Interception 0,026

28*me*.

	Soleil.	Cristal d'Islande.
0′	67	67
5	$72\frac{5}{8}$	$71\frac{1}{4}$ ..... $5\frac{5}{8} : 4\frac{1}{4} = 0{,}756$

Chaleur au soleil libre 598
Sous le cristal d'Islande 583
Rapport 0,975
Interception 0,025.

29*me*.

	Soleil.	Talc.
0′	$67\frac{1}{2}$	$67\frac{1}{2}$
5	72	$71\frac{3}{8}$ ..... $4\frac{1}{2} : 3\frac{7}{8} = 0{,}861$

Chaleur au soleil libre 590
Sous le talc - - - 584
Rapport 0,990
Interception 0,010.

30*me*.

	Soleil.	Talc aisément calcinable.
0′	50	50
5	$54\frac{3}{4}$	$53\frac{7}{8}$ ..... $4\frac{3}{4} : 3\frac{7}{8} = 0{,}816$

Chaleur au soleil libre 453
Sous le talc calcinable 448
Rapport 0,978
Interception 0,022.

31*me.*

	Soleil.	Verre rouge très obscur.	
0′	73	73	
5	$79\frac{1}{4}$	$74\frac{1}{4}$ ..... $6\frac{1}{4}$: $1\frac{1}{4}$ = 0,200	

Chaleur au soleil libre - 654
Sous le verre rouge obscur 598
Rapport 0,914
Interception 0,086.

40*me.*

	Soleil.	Verre indigo.	
0′	$61\frac{3}{4}$	$61\frac{3}{4}$	
5	$67\frac{7}{8}$	64 ..... $6\frac{1}{8}$: $2\frac{1}{4}$ = 0,367	

Chaleur au soleil libre 562
Sous le verre indigo 519
Rapport 0,923
Interception 0,077.

§ 19. Je vais encore rapporter quelques expériences, et en tirer les résultats, comme ci-dessus. Mais je dois remarquer, que dans les suivantes, il arrive souvent que les thermométres ne sont pas d'accord au point de départ. J'ignore d'où cela peut dépendre.

44*me Expérience.*

	Soleil.	Les deux fonds de verre d'un tube fermé, long de 3 pouces.	
0′	53	53	
5	59	$55\frac{3}{4}$ ..... 6 : $2\frac{3}{4}$ = 0,458	

Chaleur au soleil libre - - 490
Sous les deux fonds de verre 454
Rapport 0,927
Interception 0,073.

45*me*.

	Soleil.	Eau, et les deux fonds de verre du même tube fermé qui la contient.
0′	$52\frac{1}{4}$	$52\frac{1}{8}$
5	$58\frac{3}{4}$	55 ..... $6\frac{1}{2} : 2\frac{7}{8} = 0{,}442$

Chaleur au soleil libre - - - 490
Sous l'eau et les deux fonds de verre 449
Rapport 0,917
Interception 0,083.

47*me*.

	Soleil.	Esprit de vin, et les deux fonds de verre du même tube qui le contient.
0′	$51\frac{5}{8}$	$51\frac{5}{8}$
5	$57\frac{3}{4}$	54 ..... $6\frac{1}{8} : 2\frac{3}{8} = 0{,}388$

Chaleur au soleil libre - - - - 494
Sous l'esprit de vin et les deux fonds de verre 489
Rapport 0,889
Interception 0,111.

48*me*.

	Soleil.	*Gin*, (liqueur spiritueuse,) et les deux fonds de verre.
0′	52	52
5	$57\frac{3}{4}$	$53\frac{1}{2}$ ..... $5\frac{3}{4} : 1\frac{1}{2} = 0{,}261$

Chaleur au soleil libre - - - 480
Sous le *gin* et les deux fonds de verre 434
Rapport 0,904
Interception 0,096.

50*me*.

	Soleil.	*Crown glass* usé à l'émeri du côté exposé.
0′	67	67
5	74	$70\frac{3}{4}$ ..... $7 : 3\frac{3}{4} = 0{,}586$

Chaleur au soleil libre 614
Sous le verre - - 578
Rapport 0,941
Interception 0,059.

51*me*.

	Soleil.	*Coach glass* (glace de carrosse) usé à l'émeri du côté opposé.	
0′	$66\frac{1}{2}$	$66\frac{1}{2}$	
5	$73\frac{1}{2}$	$69\frac{1}{2}$	..... $7 : 3 = 0{,}429$

Chaleur au soleil libre 660
Sous le verre - 568
Rapport 0,861
Interception 0,139.

148*me*.

	Aux rayons invisibles du soleil libre.	A ces mêmes rayons à travers un verre blanc bleuâtre.	
0′	48	47	
5	$49\frac{3}{4}$	$48\frac{5}{8}$	..... $1\frac{3}{4} : 1\frac{5}{8} = 0{,}929$

Chaleur aux rayons libres 404
Sous le verre - - 394
Rapport 0,975
Interception 0,025.

149*me*.

	Rayons invisibles.	*Flint glass*.	
0′	$50\frac{3}{4}$	$49\frac{7}{8}$	
5	52	$51\frac{1}{8}$	..... $1\frac{1}{4} : 1\frac{1}{4} = 1{,}000$

Chaleur aux rayons libres 420
Sous le verre - - 413
Rapport 0,983
Interception 0,017.

150*me*.

	Rayons invisibles.	*Crown glass*.	
0′	$50\frac{1}{2}$	$49\frac{3}{4}$	
5	$51\frac{7}{9}$	$50\frac{7}{8}$	..... $1\frac{3}{8} : 1\frac{1}{8} = 0{,}818$

Chaleur aux rayons libres 419
Sous le verre - - - 410
Rapport 0,978
Interception 0,022.

151*me*.

	Rayons invisibles.	*Coach glass* (verre de carrosse.)
0′	$54\frac{1}{2}$	$53\frac{7}{8}$
5	$55\frac{3}{8}$	$54\frac{5}{8}$ ..... $\frac{7}{8} : \frac{3}{4} = 0{,}857$

Chaleur aux rayons libres 446
Sous le verre - - - 439
Rapport 0,984
Interception 0,016.

152*me*.

	Rayons invisibles.	Talc calcinable.
0′	$51\frac{3}{8}$	$50\frac{3}{4}$
5	$52\frac{7}{8}$	$51\frac{7}{8}$ ..... $1\frac{1}{2} : 1\frac{1}{8} = 0{,}750$

Chaleur aux rayons libres 428
Sous le talc calcinable 419
Rapport 0,979
Interception 0,021.

§ 20. Cette comparaison, entre mes résultats et ceux que l'auteur a déduit des mêmes expériences, donne lieu à quelques remarques.

Première Remarque. Nous pouvons nous faire quelque idée de l'inexactitude de mes résultats, fondés sur les deux nombres extrêmes, en calculant ainsi les trois expériences que nous avons déjà calculés sur des données plus détaillées. Quant à la 122me, (§ 11,) comme la progression est très régulière, nous sommes assurés que les deux méthodes co-incident, et toute comparaison est inutile. Dans les deux autres, au contraire, nous sommes assurés d'avance, qu'elles ne co-incident pas; et c'est cet écart qui nous intéresse.

24me Expérience.

	Soleil.	Verre blanc bleuâtre.
o′	67	67
5	78	$71\frac{1}{2}$ ..... $6 : 4\frac{1}{2} = 0{,}750$

Chaleur au soleil libre 602
Sous le verre - - 588
Rapport 0,974
Interception 0,026.

Mon résultat précédent (§ 8) donnoit précisément, ou à un huitième près, la même chaleur au soleil libre. Sous le verre elle donnoit seulement 578; ce qui est bien naturel, puisque l'échauffement sous le verre a excédé la progression dans les dernièrs temps; en conséquence, les rayons interceptés étoient exprimés par 0,038.

Le rapport des interceptions, déterminées par ces deux méthodes, est celui de 18 à 19, qui est très voisin de celui de 2 à 3. Ici donc, pour trouver l'interception résultant du calcul fondé sur toutes les données de l'expérience, il falloit augmenter l'interception déterminée par les deux nombres extrêmes, dans le rapport de 2 à 3.

25me Expérience.

	Soleil.	*Flint glass.*
o′	$69\frac{3}{4}$	$69\frac{3}{4}$
5	$75\frac{1}{4}$	$74\frac{3}{4}$ ..... $5\frac{1}{2} : 5 = 0{,}909$

Chaleur au soleil libre 619
Sous le verre - 613
Rapport 0,990
Interception 0,010.

Mon résultat précédent (§ 9) donnoit 614, au lieu de 619, pour la chaleur au soleil libre; et 604, au lieu de 613, sous le verre; et l'interception étoit 0,015, au lieu de 0,010. Ici donc encore, il auroit convenu d'augmenter l'interception, déterminée par deux nombres seulement, selon le rapport de 2 à 3, afin d'avoir l'interception résultant de toutes les données.

On doit présumer, qu'il en est de même de la plupart des autres expériences dont nous n'avons pas le détail. En appliquant cette correction à toutes celles qui sont dans ce cas, dont j'ai fait ci-dessus le calcul, (§§ 18 et 19,) il en résulteroit la table suivante, dans laquelle mes résultats sont rapprochés de ceux de l'observateur, tant pour la chaleur que pour la lumière; et où l'on remarquera, que l'interception de la chaleur, calculée selon ma méthode, (d'après la loi du § 3,) est constamment moindre que l'interception de la lumière, dont elle est une fraction qui varie entre un et sept dixièmes.

Interception de la chaleur par différentes matières.
Sur 1000 *rayons.*

Numéros des expériences.	*Au soleil.*	Interception selon la loi du § 3.	Interception selon l'observateur. Chaleur.	Lumière.
24.	Verre blanc bleuâtre -	38	250	86
25.	*Flint glass* - -	15	91	34
26.	*Crown glass* verdâtre -	49	259	203
27.	*Coach glass* (glace de carrosse) - - - -	39	214	168
28.	Cristal d'Islande - -	38	244	150
29.	Talc - - - -	15	139	90
30.	Talc aisément calcinable	33	184	288
31.	Verre rouge très obscur -	129	800	999,9
40.	Verre indigo - -	115	633	999,7
44.	Les deux fonds de verre d'un tube fermé, long de 3 pouces	109	542	204
45.	Les deux fonds de verre, et l'eau que le tube contient	124	558	211
47.	Les deux fonds de verre, et l'esprit de vin contenu	166	612	224
48.	Les deux fonds, et le *gin* contenu - -	144	739	626
50.	*Crown glass* usé à l'émeri du côté exposé - -	88	464	854
51.	*Coach glass* (glace de carrosse) usé à l'émeri du côté exposé - -	208	571	885
	Aux rayons invisibles.			
148.	Verre blanc bleuâtre -	38	71	—
149.	*Flint glass* - - - -	25	—	—
150.	*Crown glass* - - -	33	182	—
151.	*Coach glass* (glace de carrosse) - - -	24	143	—
152.	Talc calcinable - -	31	250	—
	Au feu de charbon.			
122.	Talc calciné pendant l'expérience - -	196	713	288

Mais, outre qu'il y a probablement des cas auxquels la correction aura été appliquée mal-à-propos, (cas qu'il m'est impossible de déterminer,) je crois, qu'avant de prononcer d'une manière générale sur la qualité de chaque corps, il conviendroit de les réduire tous en lames d'une égale épaisseur, et de les exposer à des chaleurs égales, par les raisons que j'ai exposé ci-dessus. (§ 13.) Mais il est probable, que ces causes d'erreur ne masquent pas entièrement la vérité.

§ 21. 2*de Remarque.* La faculté interceptante de cinq substances, rélativement aux rayons invisibles, conclue des expériences 148me et suivantes, par les deux observations extrêmes, est, selon mon résultat, (fondé sur la loi du § 3,) fort rapprochée de celle de ces mêmes substances, rélativement à tout le rayon solaire. (*Exp.* 24, et suivantes.)

Selon le résultat de l'observateur, la différence est plus considérable; elle est même infinie par rapport au *flint glass*, puisque, selon cette manière d'apprécier la transmission, les rayons invisibles ont tous traversé le *flint glass*, et n'ont point été interceptés. Ce résultat, qui paroît invraisemblable, surtout lorsqu'on a sous les yeux la suite de ces expériences, suffit seul pour ébranler la confiance en la méthode par laquelle il a été déduit.

On éprouveroit encore plus de défiance, si cette méthode venoit à présenter quelques cas, où la quantité des rayons transmis parût plus grande que celle des rayons libres. Or, ce cas peut très bien se présenter; puisqu'il suffit pour cela, qu'au 1er instant, la différence de la température du thermométre placé sous le corps interceptant, à celle du milieu où il est plongé, soit moindre que la différence de la température du thermométre exposé aux rayons libres, à celle de ces mêmes rayons. Si ce cas ne s'est pas présenté ici, c'est, sans doute, par-

ceque l'observateur avoit à dessein pris soin de mettre ces deux thermométres au même dégré initial. Cependant, cela n'a pas toujours eu lieu ; et, en conséquence, il est arrivé une fois, que les deux thermométres ont varié également pendant la durée de l'expérience. S'il tentoit de nouvelles expériences, en ayant soin de tenir, au premier instant, la température du thermométre garanti beaucoup plus basse que celle du thermométre exposé aux rayons libres, on peut prévoir qu'il arriveroit souvent, en suivant sa méthode de calcul, que la transmission paroîtroit avoir accru le nombre des rayons.

§ 22. *3me Remarque.* En jettant les yeux sur mes résultats, comparés à ceux de M. HERSCHEL, on verra que ceux-ci donnent tous des interceptions beaucoup plus fortes. Une expérience de M. PICTET* donne une interception encore plus forte, et qui surpasse toutes celles qu'indiquent les tables de M. HERSCHEL, du moins pour les verres polis et sans couleur. Un thermométre, exposé à une source de chaleur, monta de 10° ; garanti par un carreau de verre, ce thermométre baissa de 6°. Il paroît donc, que ce verre interceptoit les $\frac{3}{5}$ de la chaleur, ou 600 millièmes.

Ici l'observateur n'a point voulu limiter le temps, et paroît avoir eu dessein de laisser son thermométre atteindre la température de la source, soit libre, soit gênée ; ensorte qu'on ne peut se refuser à cette conséquence, que le verre a dérobé au thermométre plus de la moitié de la chaleur, à l'influence de laquelle on l'avoit exposé.

Ce résultat s'éloignera moins de ceux qu'on peut déduire des observations de M. HERSCHEL, si l'on a égard aux considérations suivantes. 1. Quelle que soit la faculté interceptante d'une

* Essai sur le Feu, § 52.

lame, l'interception doit croître, si l'on augmente son épaisseur. Si donc le carreau de M. PICTET étoit plus épais que les lames employées par M. HERSCHEL, la transmission devoit être moindre. Cette circonstance de l'expérience est inconnue de part et d'autre; je n'en fais mention que comme d'une simple possibilité. La suivante est moins indéterminée. 2. Dans l'expérience de M. PICTET, le verre interposé étoit probablement froid, par comparaison au thermomètre; la présence de ce corps froid, (quoiqu'à la distance de 5 pieds 7 pouces,) doit avoir eu quelque influence. 3. De plus, ce carreau interceptoit un courant d'air favorable à l'échauffement du thermomètre. 4. Enfin, la source de chaleur, absorbée en partie par le verre, n'auroit pas manqué de l'échauffer à la fin sensiblement, et cet échauffement se seroit fait sentir au thermomètre. Mais l'expérience finit probablement à cette époque; car l'observateur dut naturellement être satisfait, quand il eut obtenu le maximum de refroidissement, qui étoit l'objet unique de son attention. D'ailleurs, l'appareil de M. PICTET est tel, que l'action directe du verre échauffé ne peut se faire sentir, que lorsqu'elle est déjà assez grande.

Au contraire, dans les expériences de M. HERSCHEL, on voit des thermomètres placés à environ 2 pouces de la lame interceptante, et participant au moindre échauffement de cette lame. Il n'y a d'ailleurs aucune cause de réfroidissement; et les lames sont probablement très-minces.

Telles sont les causes auxquelles j'attribue les différences observées dans les résultats déduits des expériences de ces deux habiles physiciens; et ces considérations nous ramènent a dire, que ces résultats, de quelque facon qu'on les calcule, varieront tant qu'on ne prendra pas des lames de même épaisseur. Ils varieroient encore probablement, si l'on faisoit varier la distance

de la lame au thermométre, puisqu'on feroit varier par cela même, l'influence de la chaleur qui s'accumule dans la lame.

Du reste, la petitesse de mes résultats (fondés sur la loi du § 3) n'a rien qui puisse surprendre, puisque nous avons reconnu dès l'entrée, que nos calculs ne pouvoient nous donner qu'une limite de petitesse. (§ 14.). Il est donc très vraisemblable, que lors qu'on sera parvenu à mesurer à-part la chaleur transmise, on trouvera qu'elle est bien moindre, et l'interception bien plus grande, que nos résultats ne la présentent.

§ 23. *4me Remarque.* Et par quel moyen pourra-t-on parvenir à faire cette appréciation, à décomposer l'effet en ses deux élémens? Il me semble que ce doit être, en observant l'effet instantanée de la chaleur à travers un obstacle, et non son effet au bout d'un temps fini. Il faudra donc recourir à des thermométres très sensibles, tels que ceux d'air, employés et décrits par M. PICTET.* En voyant comment ils se comportent sous le verre, à l'instant même où celui-ci reçoit l'impression calorifique, on jugera d'abord de l'influence de la chaleur transmise, car on sait bien qu'il faut un certain temps pour que l'accumulée ait son effet; mais ce temps n'est pas suffisamment déterminé, et le phénomène varie probablement à différentes époques.

§ 24. *5me Remarque.* Tout ce que je viens de dire s'accorde fort bien avec un phénomène que M. PICTET a observé, et avec l'explication qu'il en donne. Un grand miroir concave de verre étamé, ne renvoyoit presque aucune chaleur à son foyer, sous l'influence des mêmes rayons qui, dans la même situation, élevoient le thermométre de plus de 10°, au foyer d'un miroir métallique. "Dans les miroirs de verre," dit M. PICTET,† "ce n'est point la surface antérieure qui réfléchit la plus grande

* Essai sur le Feu, § 56. † Ibid. § 67.

" partie des rayons, c'est surtout la surface métallique appliquée " derrière le verre. La chaleur, pour arriver à cette surface, a " toute l'épaisseur du verre à traverser; elle ne peut se réfléchir " sans la traverser de nouveau, et, étant ainsi doublement *tamisée* " par une substance qui ne lui laisse qu'un passage bien difficile, " il n'en échappe que peu pour agir sur le thermométre..... " mais, que devient cette chaleur ainsi interceptée par le verre? " ..... Elle reste..... dans le verre, et s'emploie à le ré- " chauffer; elle se répand dans sa substance, à raison de la " chaleur spécifique du verre, et on s'appercevroit sans doute de " son effet, si le miroir restoit longtemps exposé a l'action du " foyer calorifique." Remarquons seulement, que cette action, n'étant point concentrée au foyer, seroit peu sensible.

§ 25. *6me Remarque.* En conséquence de toutes nos distinctions et explications précédentes, je me demande, quels sont les phénomènes successifs que doit offrir un thermométre placé derrière une lame interceptante? 1. Au premier instant, la chaleur transmise doit agir; mais probablement elle n'est qu'une foible aliquote de la source de chaleur qui atteint la lame. 2. Bientôt la chaleur absorbée par la lame s'y accumule assez pour rayonner, et envoyer au thermométre des emanations calorifiques. Cette influence suit le progrès de l'échauffement de la lame. 3. Enfin la lame s'échauffe au maximum qu'elle peut atteindre; alors le thermométre se trouve dans un courant de chaleur constante, et se fixe.

§ 26. *7me Remarque.* De quelle quantité, la chaleur sous cette lame différera-t-elle finalement de la chaleur libre?

Si la lame étoit plongée toute entière dans la source de chaleur, de sorte que celle-ci l'enveloppât de toutes parts, comme un bain, on sait que la lame acquerroit enfin la température de

la source; mais, n'étant en contact avec elle que par une de ses faces, elle doit s'échauffer moins que si toutes deux lui fournissoient du feu; ensorte que, par cette raison, elle ne peut atteindre le dégré de chaleur de la source. Il y a un moment où la lame a acquis son maximum de chaleur; c'est celui où elle perd autant par ses deux surfaces, qu'elle acquiert par une seule; et ce maximum est nécessairement moindre que si elle acqueroit par toutes deux, par conséquent, moindre que la température de la source.

De plus, la chaleur réfléchie n'échauffe pas le corps qui la réfléchit; il faut donc déduire de la source de chaleur, tous les rayons réfléchis, lorsqu'il s'agit d'estimer l'échauffement de la lame interceptante.

Le thermométre placé sous le verre reçoit donc, 1. les rayons transmis instantanément; 2. les émanations de la chaleur accumulée dans le verre; mais il ne reçoit pas les rayons réfléchis; et la chaleur du verre a un maximum peu élevé.

§ 27. 8*me Remarque*. Ceci étant suffisamment éclairci, on concevra en quels cas le calcul des expériences de M. HERSCHEL, par les deux extrêmes, donnera, ou ne donnera pas, des resultats qui s'écartent de ceux qu'on auroit déduit de toutes les observations successives. Dans presque tous les cas de ce genre, il doit y avoir, en vertu de la chaleur accumulée dans la lame, un échauffement final plus grand que ne le comporte la loi. En conséquence, si l'on ne prend que les extrêmes de chaleur, (le dégré initial et le dégré final,) et qu'on suppose l'accroissement de chaleur régulier, (c'est-à-dire conforme à la loi,) on sera conduit nécessairement à trouver l'interception moindre que si on l'eût calculée par les dégrés observés aux premières minutes. C'est ce que nous avons vérifié sur les expériences 24me et

25me. Nous avons reconnu que, dans ces expériences, cette différence alloit à-peu-près à la moitié de l'interception estimée par les deux extrêmes; ensorte que ces deux résultats étoient entr'eux comme les nombres 2 et 3. (§ 20.)

Comparons maintenant, sous ce point de vue, deux sources de chaleur inégales. Nous supposerons deux expériences, où chacune de ces sources agit, d'un côté librement, de l'autre à travers la même lame interceptante. Si l'accroissement de chaleur sous le verre étoit proportionnel à celui qui a lieu sous l'influence de la source libre, il est facile de voir que le calcul de l'interception la feroit paroître plus grande à la source la plus chaude. En voici un exemple, fictif, mais propre à rendre la chose sensible.

No. I.			No. II.	
	Soleil.	Verre.	Soleil.	Verre.
0′	600	600	600	600
5	640	620	680	640
Chaleur au soleil libre		655	Chaleur au soleil libre	710
Sous le verre - -		628	Sous le verre - -	655
Interception		0,042	Interception	0,077

Il est vrai que les deux accroissemens, que j'ai supposés proportionnels, ne le sont pas; mais, comme ils augmentent et diminuent ensemble, et par la même cause, on peut bien affirmer, que la même lame fera paroître, au calcul, l'interception plus grande sous l'influence d'une source plus chaude, et réciproquement.

C'est aussi ce qu'on peut remarquer dans les expériences de M. Herschel, où, à travers les mêmes lames, on voit une chaleur de feu de charbon, d'environ 730, produire une interception d'environ 200; tandis que, dans les expériences au soleil,

une chaleur d'environ 600, n'a produit qu'une interception d'environ 30.

§ 28. 9*me Remarque.* C'est par la même cause, qu'à travers 4 verres, au feu de charbon, l'interception a paru moindre qu'à travers un seul; car, dans l'expérience des quatre verres, la chaleur du feu n'étoit que 655, au lieu que dans celles où il n'y avoit qu'un verre, elle étoit 757, 731, 782, 741. Dans celle où il y avoit deux verres, la chaleur étoit 700, moyenne entre celles que je viens de comparer, et l'interception a aussi été moyenne. C'est ce qui résulte du calcul suivant, où j'expose les expériences et leur résultats, en huitièmes de dégré, déduits selon la méthode expliquée ci-dessus, (§ 17,) en supposant, de minute en minute, la progression des différences dans le rapport de 7 à 5, parceque ce rapport est celui que nous a indiqué l'expérience 122, dont nous avons les détails, (§ 11,) et qui a été faite dans les mêmes circonstances.

Expériences faites au feu de charbon.

117*me.*

	Feu.	Verre blanc bleuâtre.
0′	528	528
5	688	568

Chaleur du feu libre 724
Sous le verre 577
Interception 0,203.

118*me.*

	Feu.	*Flint glass.*
0′	536	536
5	696	576

Chaleur du feu libre 732
Sous le verre - 585
Interception 0,201.

119*me*.

	Feu.	*Crown glass.*
0′	536	536
5	694	580

Chaleur du feu libre 729
Sous le verre - 590
Interception 0,191.

186*me*.

	Feu.	*Crown glass* usé à l'émeri du côté exposé seulement.
0′	541	541
5	718	590

Chaleur du feu libre 757
Sous le verre - 601
Interception 0,206.

137*me*.

	Feu.	*Coach glass* (glace de carrosse) usé à l'émeri du côté exposé seulement.
0′	544	540
5	697	577

Chaleur du feu libre 731
Sous le verre - 585
Interception 0,200

138*me*.

	Feu.	*Crown glass* usé à l'émeri des deux côtés.
0′	548	544
5	739	584

Chaleur du feu libre 782
Sous le verre - 593
Interception 0,244.

139*me*.

	Feu.	*Coach glass* usé à l'émeri des deux côtés.
0′	536	536
5	704	564

Chaleur du feu libre 741
Sous le verre - 570
Interception 0,231.

140*me*.

	Feu.	Les deux verres de *crown* et *coach glass* usés à l'émeri d'un côté seulement.
0′	528	528
5	688	559

Chaleur du feu libre 724
Sous les deux verres 566
Interception 0,232.

141*me*.

	Feu.	Les deux mêmes verres, usés à l'émeri des deux côtés.
0′	534	534
5	670	548

Chaleur du feu libre 700
Sous les deux verres 551
Interception 0,218.

142*me*.

	Feu.	Les quatre verres des deux expériences précédentes.
0′	528	528
5	640	539
Chaleur du feu libre -	665	
Sous les quatre verres -	541	
Interception 0,186.		

Je viens à l'exposition de cette partie de la théorie de la chaleur, dont j'ai dit que dépendoit la loi de l'échauffement, que l'observation directe a fait reconnoitre. (§ 3.)

Partie II.

§ 29. Plusieurs raisons m'engagent à suivre, dans l'exposé de la théorie que j'ai en vue, un ordre relatif à l'histoire de sa découverte. Il résultera de là, que je paroîtrai d'abord m'écarter un peu de mon sujet; mais j'y rentrerai très vîte, ou plutôt je n'en sortirai point.

Bacon proposoit cette expérience: " Les chaleurs brillantes " et radieuses sont exaltées par les verres: les chaleurs obscures " et opaques, (comme celles des pierres et des métaux, avant " d'être rougis par la force du feu,) sont elles sujettes à la même " impression ?" *

Plusieurs physiciens postérieurs avoient observé qu'un charbon ardent, placé entre deux miroirs concaves, allumoit un corps combustible à plus de 20 pieds de distance. Lambert attribuoit cet effet à la chaleur obscure, et non a la chaleur lumineuse. Il étoit conduit à penser ainsi, parce qu'un feu très ardent ne lui paroissoit donner aucune chaleur au foyer d'une lentille convexe.†

* *Instaurat.* l. 5. c. 2.

† Pyrométrie, § 378 et suiv. cité par M. De Saussure. Voyage aux Alpes, § 926.

M. DE SAUSSURE résolut de vérifier cette idée de LAMBERT, en substituant au charbon un boulet chaud, sans être rouge. Il s'adressa à M. PICTET, pour faire cette expérience, qui réussit parfaitement. Le boulet, occupant le foyer d'un des miroirs, fit monter de $10\frac{1}{2}$ dégrés le thermométre placé à l'autre foyer. Un matras d'eau bouillante, substitué par M. PICTET au boulet chaud, produisit le même effet, quoiqu'avec moins d'intensité.*

Ces expériences prouvérent incontestablement, que la chaleur étoit susceptible d'être réfléchie, sous la même loi que la lumière. M. PICTET a prouvé de plus, que la vîtesse de la chaleur est si grande en ce cas, qu'elle parcourt 69 pieds, dans un instant sensiblement indivisible.†

§ 30. Ces faits, quelque curieux et importans qu'ils soient, ne forcent peut-être pas le physicien à se décider sur la nature de l'agent qui produit la chaleur, et en particulier sur le moyen par lequel s'établit et se maintient l'équilibre de température entre deux corps, ou entre deux espaces voisins. On se contentoit donc d'exprimer par le mot de *tension*, ou par quelque autre équivalent, l'espèce d'effort par lequel il s'opéroit. Ainsi, lorsque deux espaces sont inégalement chauds, la tension supérieure du plus chaud, l'emportant sur celle de l'autre, amène enfin un état dans lequel les deux tensions sont égales, et se balancent. Et, quoique ce langage n'offrît à l'esprit qu'une conception indéterminée, on se crut obligé de s'en contenter, et de la recevoir comme une loi de la nature. Cette loi étoit d'ailleurs semblable à celle qu'on observe dans les fluides élastiques plus grossiers, par une suite de la compression qu'ils éprouvent. On ne savoit pas si le feu étoit précisément de même nature; mais cette com-

* Voyage aux Alpes, § 926. † Essai sur le Feu, § 64.

paraison servoit à satisfaire l'esprit, et paroissoit en quelque sorte éclairer le phénomène.

§ 31. Une expérience nouvelle vint tirer les physiciens de leur sécurité, et dut leur faire sentir l'insuffisance du langage convenu, dont ils s'étoient fait une habitude. BACON l'avoit indiquée. " La chaleur, par les verres," dit-il, " acquiert de l'intensité; en " est-il de même du froid?"* C'est de lui, probablement, que quelques auteurs subséquens avoient emprunté la même idée.† Mais cette idée étoit restée sans exécution, jusqu'à l'époque, encore récente, où M. PICTET l'a réalisé. M. BERTRAND, Professeur de Mathématiques, lui en suggéra l'idée; et voici comment M. PICTET rend compte de l'expérience. " Je disposai l'ap- " pareil précisément comme pour la réflexion de la chaleur; " j'employai les deux miroirs d'étain, à la distance de $10\frac{1}{2}$ pieds " l'un de l'autre. Au foyer de l'un étoit un thermométre d'air, " qu'on observoit avec les précautions requises; et au foyer de " l'autre, un matras plein de neige.—A l'instant où le matras " fut en expérience, le thermométre placé à l'autre foyer des- " cendit de plusieurs dégrés; il remonta dès qu'on enleva le " matras.—Après avoir remis le matras au foyer, et fait ainsi " descendre le thermométre jusqu'à un certain dégré, où il de- " meura stationnaire, je versai de l'acide nitreux sur la neige; et " le froid ainsi produit, fit à l'instant descendre le thermométre " de 5 à 6 dégrés plus bas."*

§ 32. A la vue de ce résultat, M. PICTET éprouva d'abord

* *Instaurat.* l. 5. c. 2.

† " Les miroirs ardens concentrent la chaleur; peuvent-ils concentrer le froid?" *Logique de* FELICE, T. II. p. 62. J'ai ouï dire (mais je n'en ai point la preuve) que l'auteur de cette logique avoit fait usage des cahiers du célebre Professeur CRAMER.

* Essai sur le Feu, § 69.

quelque surprise; mais il n'hésita point à prononcer, que cette réflexion du froid n'étoit qu'apparente, et qu'elle ne pouvoit être que la réflexion de la chaleur, en sens inverse.

§ 33. Cependant, aucune explication fondée sur les idées de tension, de pression, d'équilibre sans mouvement, ne pouvoit faire comprendre comment cette marche inverse de la chaleur étoit déterminée. En effet, les miroirs, l'air, et tous les corps voisins du matras froid, étant tous entr'eux à même température, doivent, selon ces systêmes, lâcher leur chaleur vers ce gouffre, et non dans aucune autre direction. On n'y voit point de raison pour qu'un rayon parte du thermomêtre, et se porte vers le miroir dont il occupe le foyer.

§ 34. Accoutumé dès long-temps à envisager le feu sous un autre aspect, j'exposai ces difficultés, et je tâchai d'attirer l'attention des physiciens sur cet objet, dans un mémoire sur *l'Equilibre du Feu*,* et dans mes *Recherches sur la Chaleur*.† Ces écrits sont, si je ne me trompe, les premiers où l'on ait proposé de substituer un équilibre mobile, à l'équilibre immobile que les physiciens ont coutume d'admettre en cette matière; et la conséquence de cette substitution fut, que le phénomène de la réflexion du froid s'expliqua aussi aisément, et aussi pleinement, que celui de la réflexion de la chaleur. C'est, je pense, un caractère de vérité; car on sent bien, que ces deux faits sont homogènes, et qu'une bonne théorie doit les expliquer à la fois, et les comprendre, pour ainsi dire, sous une même formule. Qu'il me soit permis de rappeler ici cette théorie, que j'ai eu la satisfaction de voir adopter par M. PICTET,‡ et par d'autres bons juges. Peu

* Journal de Physique, Avril, 1791. † Publiées à Genève, en 1792.

‡ Bibl. Brit. Sc. et Arts, T. IV. p. 30, et ailleurs.

de mots suffisent pour en faire saisir le principe ; c'est le seul but que je me propose ici.

§ 35. Le feu est un fluide discret, agité : chaque molécule de feu libre est mue avec une grande vîtesse ; l'une se meut dans un sens, l'autre dans l'autre, de sorte qu'en tout sens, un corps chaud émet des rayons calorifiques ; et ces molécules sont assez écartées les unes des autres, pour que deux ou plusieurs courans puissent s'entrecroiser, comme la lumière, sans se troubler mutuellement dans leur cours. Cette constitution du feu étant bien conçue, si l'on feint deux espaces voisins où il abonde, on verra, qu'entre ces espaces il y a de continuels échanges. Si, dans les deux espaces, le feu est également abondant, les échanges seront égaux, il y aura équilibre. Si l'un des espaces contient plus de feu que l'autre, les échanges seront inégaux ; le moins chaud recevra plus de molécules ignées qu'il n'en donnera ; et, après un temps suffisant, la répétition continuelle de ces échanges retablira l'équilibre.

§ 36. De ces principes découlent toutes les lois de la chaleur croissante et décroissante ; en particulier celle qui a servi de base à nos calculs comparatifs de la marche de deux thermométres exposés à une même source de chaleur, l'un sous une lame interceptante, et l'autre sans aucun obstacle. (§ 3)

En effet, supposons un corps placé dans un milieu plus chaud que lui, et que ce milieu jouisse toujours d'une température constante ; on doit considerer la chaleur du milieu comme composée de deux parties, l'une égale à celle du corps, l'autre égale à la différence des deux chaleurs. Quant à la première, les échanges sont égaux entre le corps et le milieu, il y a équilibre. L'excès de chaleur du milieu peut donc être considéré seul ; et, relativement à cet excès, le corps est absolument froid. Supposons,

qu'en une seconde le corps reçoive la $\frac{1}{10}$me partie de ce feu; à la fin de de cette seconde, l'excès ne sera plus que de $\frac{9}{10}$. La $\frac{1}{10}$me de ce nouvel excès passera dans le corps pendant le cours de la 2me seconde, et l'excès sera réduit aux $\frac{9}{10}$mes des $\frac{9}{10}$mes. On voit, en suivant ce raisonnement, qu'à la fin de la 3me seconde, l'excès sera la 3me puissance de $\frac{9}{10}$; et ainsi de suite; de manière que, (conformément à la loi observée) les temps croissant selon une progression arithmétique 0, 1, 2, 3, &c. les différences décroissent selon une progression géometrique 1, $\frac{9}{10}$, $(\frac{9}{10})^2$, $(\frac{9}{10})^3$, &c.

On déduit, avec la même facilité, la même loi de réfroidissement, pour le corps plongé dans un milieu plus froid que lui.*

C'est ainsi que la vraie théorie de la chaleur, fondée sur des faits totalement différens de ceux par lesquels RICHMANN a prouvé cette loi, nous y ramène nécessairement.

§ 37. Nous avons vu, dans la 1re Partie, l'application de ce principe. Sous ce point de vue, les expériences de M. HERSCHEL acquièrent un grand intérêt, non seulement en confirmant la loi, mais en déterminant le quotient de la progression des différences dans l'échauffement de ses thermomètres; ce qui ne peut manquer d'exciter sur cet objet l'attention des observateurs, et de donner des idées très précises sur le dégré de sensibilité de l'instrument qu'on emploie.

Cette remarque m'engage à ajouter encore ici le calcul d'une expérience de même genre, faite à la lumière réfléchie d'une chandelle. Cette expérience est rapportée dans un mémoire précédent du même auteur, lié étroitement avec celui que j'ai discuté.†

* Recherches sur la Chaleur, § 19.

† Trans. Phil. pour 1800, p. 297. Exp. 2.

	Dégrés observés.	Dégrés calculés par le rapport de 65 à 51.
0′	432	432
1	440	440
2	448	446
3	456	451
4	458	455
5	458	458

La chaleur du rayon étoit ici de 469 huitièmes de dégré.

Les autres expériences de ce mémoire ne peuvent pas être aisément soumises au calcul, parce que, dans plusieurs, les temps ne sont pas en progression arithmétique; et que, dans d'autres, la chaleur du lieu varioit pendant le cours de l'expérience, indépendamment de celle qui étoit communiquée immédiatement par la source; ce qui trouble tous les résultats.

§ 38. La théorie exposée ci-dessus, (§ 35,) explique la réflexion du froid précisément comme la réflexion du chaud, sans plus ni moins de difficulté. Concevez, dans l'appareil du double miroir, deux thermomètres, placés, l'un à un foyer, l'autre à l'autre; et d'abord, que ces deux thermomètres soient au même dégré. Il y a équilibre; le feu émis par chacun, et renvoyé à l'autre, en vertu d'une double reflexion, se trouve exactement compensé par le feu que l'autre lui renvoie par la même voie, mais en sens contraire. Maintenant, concevons que l'un des thermomètres hausse ou baisse; aussitot (les échanges étant inégaux) l'autre haussera ou baissera conformément.

§ 39. Cette théorie présente, en tout échauffement, trois espèces de chaleur. La 1re est celle qui est immédiatement reçue, dans un instant donné, par le corps qui s'échauffe. La 2de est la chaleur accumulée, et emmagazinée, dans ce même corps, en

vertu de l'échauffement qui a eu lieu dans les instans précédens. La 3me est la chaleur rayonnante, qui est l'effet des deux précédentes, et qui sort incessamment du corps, à mesure que les autres y entrent. La considération de ces trois chaleurs distinctes, a de l'influence dans plusieurs phénomènes, surtout dans la météorologie. J'ai eu occasion de faire remarquer, que l'estimation de la température des saisons en dépend.*

En un mot, le nombre des faits auxquels cette théorie s'applique, est assez considérable pour inspirer quelque confiance; et je ne sais pas voir quelle difficulté réelle elle présente. †

§ 40. A la vérité, quelques physiciens semblent disposés à substituer dans la nature, les fluides continus aux fluides discrets, et le mouvement ondulatoire à celui de translation. Je pourrois dire, comme il est assez commode de faire, que je ne détermine rien à cet égard, et qu'on n'a qu'à mettre partout, dans ce qui precéde, des ondes qui se croisent, au lieu de courants et de particules distinctes. Mais je ne crois pas cette substitution légitime; et, sans parler de plusieurs raisons qui la combattent, il en est une générale, qui seule devroit, à ce qu'il me semble, la faire rejetter: les agens continus obstrueroient l'univers, et s'opposeroient aux mouvemens libres et rapides qu'on y observe.

§ 41. Pour me résumer, je dis, 1. Que l'effet d'une source de chaleur constante sur le thermométre, en un temps limité, n'est pas proportionnel à la chaleur de la source. 2. Qu'on a

* Réflexions sur la Chaleur solaire, &c. Journ. de Phys. Fevrier, 1793.

† Je n'ai point parlé de la communication de la chaleur par les corps qui la conduisent; et je ne me suis point occupé, dans ce mémoire, du feu latent et combiné; ce n'étoit pas mon sujet. Il est du reste facile a voir, que ces effets ne contrarient en rien la théorie que j'ai exposé; mais s'allient, au contraire, très bien avec les phénomènes de la chaleur rayonnante et libre.

néanmoins un moyen de conclure la chaleur de la source, de son effet sur le thermométre; parcequ'on connoît la loi que suit cet effet, dans ses accroissemens successifs. 3. Que cette méthode est la seule qu'on doive employer, lorsqu'il s'agit de comparer deux sources de chaleur, d'après leur effet en un temps limité, moindre que celui qui est requis pour le maximum de l'effet. 4°. Que, lorsqu'il s'agit de chaleur transmise, il faut distinguer celle qui est transmise immédiatement, de celle que le corps transmettant y ajoute dès qu'il s'échauffe. 5. Que, lorsqu'on néglige cette distinction, l'interception de chaleur attribuée à la lame n'est qu'une limite de petitesse; ensorte qu'il reste indécis, si l'interception n'a pas été beaucoup plus grande, ou même totale. 6. Qu'en appliquant ces principes aux expériences de M. Herschel, l'appréciation devient plus exacte, mais dépend néanmoins de quelques circonstances accessoires, et jusqu'ici indéterminées. 7°. Que, dans ces mêmes expériences, la différence apparente entre l'interception de la chaleur et celle de la lumière, par les mêmes matières, n'établit aucune conclusion légitime sur la différence ou l'identité de la lumière et de la chaleur. 8. Que la loi mentionnée ci-dessus (et que j'ai énoncé au § 3) n'est pas seulement prouvée par l'expérience directe, mais par son accord avec la vraie théorie de la chaleur. 9. Que cette théorie est établie sur des faits variés, tout-à-fait différens de cette loi, en particulier sur la réflexion du froid; et qu'elle est la seule qui s'accorde avec les phénomènes généraux de la nature.

XVII. *Of the Rectification of the Conic Sections. By the Rev.* John Hellins, *B. D. F. R. S. and Vicar of Potter's-Pury, in Northamptonshire.*

Read July 8, 1802.

PART I.

Of the Rectification of the Hyperbola: containing several new Series for that Purpose; together with the Methods of computing the constant Quantities by which the ascending Series differ from the descending ones.

INTRODUCTION.

THE conic sections are a part of geometry so requisite in mensuration, in optics, astronomy, and other branches of natural philosophy, that the properties of these curves have been much studied in the course of the last hundred and fifty years; and there is hardly a writer on fluxions, of any note, who has not treated of their rectification. It may therefore seem, that little is now left to the industry of the present and future generations, in this part of the mathematics, but the proper application of theorems already investigated. Yet, while we admire the skill, and praise the industry, of those who have discovered new truths, or thrown new light on old ones, within that period, we shall do well to recollect, that it is now no more than one hundred and thirty-seven years, since the two great discoveries of *fluxions* and *infinite series* were made by Sir ISAAC NEWTON; and that the observation of the late Mr. EMERSON, respecting

the state of fluxions in his time, is in a great measure applicable to it in ours; viz. "*If arts and sciences of many hundred* "*years standing receive daily improvements and additions, it can-* "*not be supposed that this most sublime art of all, found out but* "*yesterday, can be arrived at perfection all on a sudden. If this* "*art be so exceedingly useful and valuable, it certainly deserves* "*the pains and attention of the learned mathematicians.*"*—And indeed, whoever considers the great number of mathematical and physico-mathematical problems which are solved by means of fluxions and series only, the several different ways in which series may be applied to the solution of the same problem, the fewness of those who employ themselves at all about these abstract sciences, and the still smaller number of those who have skill, leisure, and resolution enough to attempt any improvement in them; I say, whoever duly considers these things, (even without making allowance for the want of patronage which the liberal arts have of late years experienced,) will see reason to think, that many ages must yet elapse, before this most sublime and extensively useful method of computation will receive all the improvements of which it is capable. He will perceive, that, of the large field opened by Sir ISAAC NEWTON, a considerable part is still covered with briars and thorns. He will have no doubt, that the mine is not yet exhausted, but that, although the first workers of it have carried away the largest and most brilliant diamonds, enough still remain to reward the labour of those who shall have the resolution to dig deeper, and the patience of those who shall yet carefully sift the rubbish which has been thrown up by former adventurers.

The subject of the following sheets was first offered to my

* Preface to his Fluxions.

consideration in the year 1770, when a problem requiring the rectification of a large portion of an equilateral hyperbola was proposed in a periodical work; which problem I then solved by means of descending series; but, for want of an easy method of correcting the fluent so found, I laid it aside for the exercise of maturer judgment. Afterwards, the subject was resumed at different times, as leisure permitted, and put into nearly the same form in which it now appears, in the year 1795; since which time, till now, the duties of my station, and unexpected occurrences, have left me no opportunity to revise my papers.

As the investigation of the following theorems is very obvious and easy, I thought it probable that they might have been discovered by some other person before me; yet, upon perusal of Dr. HUTTON's *mathematical and philosophical Dictionary*, lately published, I find but one of them. And since, in the compilation of that work, as the learned and industrious author professes, "*Not only most of the encyclopedias already extant, and the* "*various transactions of the learned societies throughout Europe,* "*have been carefully consulted, but also all the original works, of* "*any reputation, which have hitherto appeared upon these sub-* "*jects,*"* I therefore conclude that all the rest are new.

The subject of this Paper is naturally divided into three sections: the first containing the investigations of the several series; the second, the methods of computing the constant quantities by which the ascending series differ from the descending ones; and the third, examples of their use, by way of illustration. But, for more convenient reference, I have further divided it into articles, or minor sections.

* See Dr. HUTTON's Address to the public, on the publication of the first part of the *Dictionary* above mentioned, in 1795; and preface to the *Dictionary*.

SECT. I. *The Investigations of the several Series.*

1. That the following processes may not be incumbered with symbols, and that the rate of convergency of the series obtained therefrom may be the more obvious, let the transverse axis of any hyperbola be called $2a$, and the conjugate axis 2; (by which notation, any ratio that these two lines can possibly have to each other may be expressed;)* let the abscissa be called x, the corresponding ordinate to the axis, y, and the length of the curve from the vertex to the ordinate, z. Then, by the well-known property of the curve, we have $2ax + xx = aayy$; from which x is found $= a\sqrt{(1+yy)} - a$, and $\dot{x} = \frac{ay\dot{y}}{\sqrt{(1+yy)}}$, and $\dot{z} = \sqrt{(\dot{y}\dot{y} + \dot{x}\dot{x})} = \dot{y}\sqrt{\left(1 + \frac{aayy}{1+yy}\right)} = \frac{\dot{y}\sqrt{(1+yy+aayy)}}{\sqrt{(1+yy)}}$; which equation, by writing ee for $1 + aa$, will become $\dot{z} = \frac{\dot{y}\sqrt{(1+eeyy)}}{\sqrt{(1+yy)}}$.

2. Now, the fluent of the expression on the right-hand side of the last equation may be taken in different series, according as the numerator or denominator of it is converted into series, and according as 1, $eeyy$, or yy, is made the leading term. By converting the numerator, $\sqrt{(1+eeyy)}$, into series, making 1 the leading term, we get $\dot{z} = \frac{\dot{y}}{\sqrt{(1+yy)}} \times : 1 + \frac{eeyy}{2} - \frac{e^4y^4}{2.4} + \frac{3e^6y^6}{2.4.6} - \frac{3.5e^8y^8}{2.4.6.8}$, &c. and then, by taking the fluents of $\frac{\dot{y}}{\sqrt{(1+yy)}}$, $\frac{\dot{y}yy}{\sqrt{(1+yy)}}$, $\frac{\dot{y}y^4}{\sqrt{(1+yy)}}$, &c. and denoting them by A, B, C, &c. respectively, we shall have

* With respect to homogeneity, about which some have shown more scrupulosity than discernment, I shall add a few words in a subsequent part.

$$A = \text{H. L. of } \overline{y + \sqrt{(1+yy)}},$$
$$B = \frac{y\sqrt{(1+yy)} - A}{2},$$
$$C = \frac{y^3\sqrt{(1+yy)} - 3B}{4},$$
$$D = \frac{y^5\sqrt{(1+yy)} - 5C}{6},$$
$$E = \frac{y^7\sqrt{(1+yy)} - 7D}{8},$$
&c. &c.

And, lastly, by multiplying these quantities by their proper coefficients, we obtain (THEOREM I,)

$z = A + \frac{ee}{2}B - \frac{e^4}{2.4}C + \frac{3e^6}{2.4.6}D - \frac{3.5e^8}{2.4.6.8}E$, &c. where it is manifest that, unless the quantities B, C, D, &c. decrease in the ratio of ee to 1, the series will at last cease to converge; or, in other words, if yy be greater than $\frac{1}{ee}$, the terms of the series, at a great distance from the first, will diverge. And, of the nine theorems now produced, this is the only one that I have found in any other book.

3. But, by converting $\sqrt{(1+yy)}$, the denominator of the fraction in the fluxionary equation in Art. I. into series, making 1 the leading term, we have $\dot{z} = \dot{y}\sqrt{(1+eeyy)} \times : 1 - \frac{yy}{2} + \frac{3y^4}{2.4} - \frac{3.5y^6}{2.4.6} + \frac{3.5.7y^8}{2.4.6.8}$, &c. and, by taking the fluents of $\dot{y}\sqrt{(1+eeyy)}$, $\dot{y}yy\sqrt{(1+eeyy)}$, $\dot{y}y^4\sqrt{(1+eeyy)}$, &c. and calling them A, B, C, &c. we shall have

$$A = \frac{y}{2}\sqrt{(1+eeyy)} + \frac{1}{2e} \times \text{H. L. } \overline{ey + \sqrt{(1+eeyy)}},$$
$$B = \frac{y(1+eeyy)^{\frac{3}{2}} - A}{4ee},$$
$$C = \frac{y^3(1+eeyy)^{\frac{3}{2}} - 3B}{6ee},$$
$$D = \frac{y^5(1+eeyy)^{\frac{3}{2}} - 5C}{8ee},$$
$$E = \frac{y^7(1+eeyy)^{\frac{3}{2}} - 7D}{10ee},$$
&c. &c.

And then, by multiplying these quantities by their respective coefficients, we obtain (THEOREM II,)

$$z = A - \tfrac{1}{2}B + \tfrac{3}{2.4}C - \tfrac{3.5}{2.4.6}D + \tfrac{3.5.7}{2.4.6.8}E, \text{ \&c.}$$

which series will converge till y becomes greater than 1; and consequently is a better series than that above found, which ceases to converge when y becomes greater than $\frac{1}{e}$. But, when y is much greater than 1, each of these series will diverge very swiftly; and, notwithstanding they are of that form which admits of a transformation to others which will converge, still, even by that means, their values will not be obtained without great labour. But here we shall have the pleasure of finding series which will quickly answer the purpose. For,

4. By converting the denominator, $\sqrt{(yy+1)}$, into series, making yy the leading term, we get $\dot{z} = \dot{y}\sqrt{(ee\,yy+1)} \times : \frac{1}{y} - \frac{1}{2y^3} + \frac{3}{2.4y^5} - \frac{3.5}{2.4.6y^7} + \frac{3.5.7}{2.4.6.8y^9}$, &c.

And here, again, by denoting the fluents of $\frac{\dot{y}\sqrt{(ee\,yy+1)}}{y}$, $\frac{\dot{y}\sqrt{(ee\,yy+1)}}{y^3}$, $\frac{\dot{y}\sqrt{(ee\,yy+1)}}{y^5}$, &c. by A, B, C, &c. respectively, we shall have $A = \sqrt{(ee\,yy+1)} + \text{H. L.}\frac{\sqrt{(ee\,yy+1)}-1}{ey}$,

$$B = \frac{-(ee\,yy+1)^{\frac{1}{2}}}{2yy} + \frac{eeA}{2},$$

$$C = \frac{-(ee\,yy+1)^{\frac{1}{2}}}{4y^4} - \frac{eeB}{4},$$

$$D = \frac{-(ee\,yy+-)^{\frac{1}{2}}}{6y^6} - \frac{3eeC}{6},$$

$$E = \frac{-(ee\,yy+1)^{\frac{1}{2}}}{8y^8} - \frac{5eeD}{8},$$

&c. &c.

and then, (THEOREM III,)

$$z = \begin{cases} A - \frac{1}{2}B + \frac{3}{2.4}C - \frac{3.5}{2.4.6}D + \frac{3.5.7}{2.4.6.8}E, \text{ \&c.} \\ -d: \end{cases}$$

which series will converge the swifter the greater y is in comparison of 1, but will diverge when y is less than 1. It also wants a correction, (here denoted by the letter d,) which shall be given in its due place. This series then, when y becomes great in comparison of 1, will converge very swiftly, and becomes useful in those cases where the ascending series above investigated fail.

But, since the value of z may be expressed in another descending series, it will be proper to consider that also.

5. The expression $\frac{\dot{y}\sqrt{(1+ee\,yy)}}{\sqrt{(1+yy)}}$ is evidently $= \frac{e\dot{y}y}{\sqrt{(1+yy)}}\sqrt{\left(1+\frac{1}{ee\,yy}\right)}$, which, by converting $\sqrt{\left(1+\frac{1}{ee\,yy}\right)}$ into series, making 1 the leading term, becomes $\frac{e\dot{y}y}{\sqrt{(1+yy)}} \times : 1 + \frac{1}{2ee\,yy} - \frac{1}{2.4e^4y^4} + \frac{3}{2.4.6e^6y^6} - \frac{3.5}{2.4.6.8e^8y^8}$, &c. Here, the fluent of $\frac{e\dot{y}y}{\sqrt{(1+yy)}}$, the first term of the series, is $e\sqrt{(1+yy)}$; and, calling the fluents of $\frac{\dot{y}}{y\sqrt{(1+yy)}}$, $\frac{\dot{y}}{y^3\sqrt{(1+yy)}}$, $\frac{\dot{y}}{y^5\sqrt{(1+yy)}}$, &c. A, B, C, &c. respectively, we have

$$A = \text{H. L.}\frac{\sqrt{(yy+1)}-1}{y},$$

$$B = \frac{-\sqrt{(yy+1)}}{2yy} - \frac{A}{2},$$

$$C = \frac{-\sqrt{(yy+1)}}{4y^4} - \frac{3B}{4},$$

$$D = \frac{-\sqrt{(yy+1)}}{6y^6} - \frac{5C}{6},$$

&c. &c.

and thence, (THEOREM IV,)

$$z = \begin{cases} e\sqrt{(yy+1)} \\ +\frac{1}{2e}A - \frac{1}{2.4e^3}B + \frac{3}{2.4.6e^5}C - \frac{3.5}{2.4.6.8e^7}D, \text{ &c.} \\ -d: \end{cases}$$

which series will converge the swifter, the greater y is in com-

parison of 1, and has an evident advantage over the last, in that it converges by the powers of ee, as well as by those of yy; so that its convergency will not cease, till the quantities B, C, D, &c. increase in the ratio of 1 to ee, that is, when y becomes equal to, or less than, $\frac{1}{e}$. This series, therefore, will be very useful for the greatest part of the hyperbola, when it is corrected by the constant quantity here denoted by d, the value of which is attainable several ways, as will appear in the next section.

6. These four theorems, or indeed two of them only, are sufficient for the rectification of any portion whatever of any conical hyperbola. Yet, as I have discovered several other series for that purpose, which are more convenient in particular cases, and of which some are useful in computing the constant quantity above denoted by d, (by which the ascending series differ from the descending ones,) it may be proper now to give the investigations of them also.

7. Put $1 + ee\,yy = uu$; then will yy be $= \frac{uu-1}{ee}$, and $1 + yy = \frac{uu+ee-1}{ee} =$ (by the notation in Art. 1, where ee was put $= aa + 1$,) $\frac{uu+aa}{ee}$, and therefore $\sqrt{(1+yy)} = \frac{\sqrt{(uu+aa)}}{e}$, and thence $\sqrt{\left(\frac{1+ee\,yy}{1+yy}\right)} = \frac{eu}{\sqrt{(uu+aa)}}$. Moreover, $\dot{y}$ will be $= \frac{\dot{u}u}{e\sqrt{(uu-1)}}$, and we shall have $\dot{y}\sqrt{\left(\frac{1+ee\,yy}{1+yy}\right)} = \dot{z} = \frac{\dot{u}\,uu}{\sqrt{(uu-1)}\times\sqrt{(uu+aa)}} = \frac{\dot{u}\,u}{\sqrt{(uu-1)}\times\sqrt{\left(1+\frac{aa}{uu}\right)}} = \frac{\dot{u}\,u}{\sqrt{(uu-1)}} \times : 1 - \frac{aa}{2uu} + \frac{3a^4}{2.4u^4} - \frac{3.5a^6}{2.4.6u^6} + \frac{3.5.7a^8}{2.4.6.8u^8}$, &c. Now the fluent of $\frac{\dot{u}\,u}{\sqrt{(uu-1)}}$ is $\sqrt{(uu-1)}$; and, if the fluents of $\frac{\dot{u}}{u\sqrt{(uu-1)}}$, $\frac{\dot{u}}{u^3\sqrt{(uu-1)}}$, $\frac{\dot{u}}{u^5\sqrt{(uu-1)}}$, &c. are denoted by A, B, C, &c. respectively, we shall have

$A =$ circ. arch, rad. being 1, and sec. u,

$B = \frac{\sqrt{(uu-1)}}{2uu} + \frac{A}{2}$,

$C = \frac{\sqrt{(uu-1)}}{4u^4} + \frac{3B}{4}$,

$D = \frac{\sqrt{(uu-1)}}{6u^6} + \frac{5C}{6}$,

&c. &c.

And, lastly, by multiplying these quantities by their proper coefficients, and collecting the several terms in due order, we shall have (THEOREM V,)

$$z = \sqrt{(uu-1)} - \frac{aa}{2}A + \frac{3a^4}{2.4}B - \frac{3.5a^6}{2.4.6}C + \frac{3.5.7a^8}{2.4.6.8}D, \text{ \&c.}$$

Here it is remarkable, that the terms $\frac{\sqrt{(uu-1)}}{2uu}$, $\frac{\sqrt{(uu-1)}}{4u^4}$, $\frac{\sqrt{(uu-1)}}{6u^6}$, &c. which enter into the values of B, C, D, &c. always decrease while y increases from 0 *ad infinitum*; and indeed decrease more swiftly than the terms of either of the descending series in the preceding articles; and therefore this series may be used for computing the length of any portion of the hyperbola. For although the terms of it, taken at a great distance from the first, will diverge by the powers of aa, when a is greater than 1, yet, as the signs of these terms are alternately + and −, it admits of an easy transformation into another series, which will always converge by the powers of $\frac{aa}{1+aa}$. It also wants no correction; in consequence of which it has a peculiar use, which will appear in the next section.

8. But the fluxionary expression $\frac{\dot{u}\,uu}{\sqrt{(uu-1)} \times \sqrt{(uu+aa)}}$, obtained in the preceding Art. is $= \frac{\dot{u}\,u}{\sqrt{(uu+aa)} \times \sqrt{\left(1-\frac{1}{uu}\right)}} = \frac{\dot{u}\,u}{\sqrt{(uu+aa)}} \times$: $1 + \frac{1}{2uu} + \frac{3}{2.4u^4} + \frac{3.5}{2.4.6u^6} + \frac{3.5.7}{2.4.6.8u^8}$, &c. Here the fluent

of $\frac{\dot{u}}{\sqrt{(uu+aa)}}$ is $\sqrt{(uu+aa)}$; and, if the fluents of $\frac{\dot{u}}{u\sqrt{(uu+aa)}}$, $\frac{\dot{u}}{u^3\sqrt{(uu+aa)}}$, $\frac{\dot{u}}{u^5\sqrt{(uu+aa)}}$, &c. are denoted by A, B, C, &c. we shall have

$$A = \frac{1}{a}\text{ H. L. }\frac{\sqrt{(uu+aa)}-a}{u},$$

$$B = \frac{-\sqrt{(uu+aa)}}{2aauu} - \frac{A}{2aa},$$

$$C = \frac{-\sqrt{(uu+aa)}}{4aau^4} - \frac{3B}{4aa},$$

$$D = \frac{-\sqrt{(uu+aa)}}{6aau^6} - \frac{5C}{6aa},$$

&c. &c.

And, by multiplying these quantities by their proper coefficients, and collecting the products together, we shall have (THEOREM VI,)

$$z = \begin{cases} \sqrt{(uu+aa)} + \frac{1}{2}A + \frac{3}{2.4}B + \frac{3.5}{2.4.6}C + \frac{3.5.7}{2.4.6.8}D, \text{ &c.} \\ -d. \end{cases}$$

Here also, the terms $\frac{\sqrt{(uu+aa)}}{2aauu}$, $\frac{\sqrt{(uu+aa)}}{4aau^4}$, $\frac{\sqrt{(uu+aa)}}{6aau^6}$, &c. which are component parts of this series, always decrease while y increases from o *ad infinitum;* and therefore the length of any portion whatever of the hyperbola may be computed by this series also, when the value of the constant quantity d, to be taken from it, is known. But the case to which this theorem ought to be applied is, when y is equal to, or greater than 1. And it has an advantage over some of the descending series, in that the terms $\frac{A}{2}$, $\frac{3}{4}B$, $\frac{5}{6}C$, &c. are divided by aa, as will appear in the use of it.

9. When $a = 1$, that is, when the hyperbola is equilateral, the fluxionary equation in Article 7 becomes $\dot{x} = \frac{\dot{u}uu}{\sqrt{(uu-1)}\times\sqrt{(uu+1)}}$

$= \frac{\dot{u}\,uu}{\sqrt{(u^4-1)}} = \frac{\dot{u}}{\sqrt{\left(1-\frac{1}{u^4}\right)}} = \dot{u} + \frac{\dot{u}}{2u^4} + \frac{3\dot{u}}{2.4u^8} + \frac{3.5\dot{u}}{2.4.6u^{12}} + \frac{3.5.7\dot{u}}{2.4.6.8u^{16}}$,

&c.; the correct fluents of which are (THEOREM VII,)

$z = \begin{cases} u - \frac{1}{2.3u^3} - \frac{3}{2.4.7u^7} - \frac{3.5}{2.4.6.11u^{11}} - \frac{3.5.7}{2.4.6.8.15u^{15}}, \text{ &c.} \\ -d. \end{cases}$

Which series is better adapted to this case than either of the preceding ones, in that it is much simpler, and converges twice as fast. And the correction of it is easily attainable by various methods.

10. But the original fluxionary equation in Art. 1, admits of a conversion into series, two different ways from any of those which have yet been taken. For, by the Binomial theorem, $\dot{y}\sqrt{\left(1+\frac{aa\,yy}{1+yy}\right)}$ is $= \dot{y} + \frac{aa}{2}\cdot\frac{\dot{y}\,yy}{1+yy} - \frac{a^4}{2.4}\cdot\frac{\dot{y}\,y^4}{(1+yy)^2} + \frac{3a^6}{2.4.6}\cdot\frac{\dot{y}\,y^6}{(1+yy)^3} - \frac{3.5a^8}{2.4.6.8}\cdot\frac{\dot{y}\,y^8}{(1+yy)^4}$, &c. where, putting A, B, C, &c. for the fluents of $\frac{\dot{y}\,yy}{1+yy}$, $\frac{\dot{y}\,y^4}{(1+yy)^2}$, $\frac{\dot{y}\,y^6}{(1+yy)^3}$, &c. respectively, we have

$A = y -$ circ. arch, rad. 1, and tang. y,

$B = -\frac{y^3}{2(1+yy)} + \frac{3A}{2}$,

$C = -\frac{y^5}{4(1+yy)^2} + \frac{5B}{4}$,

$D = -\frac{y^7}{6(1+yy)^3} + \frac{7C}{6}$,

&c. &c.

and thence (THEOREM VIII,)

$z = y + \frac{aa}{2}A - \frac{a^4}{2.4}B + \frac{3a^6}{2.4.6}C - \frac{3.5a^8}{2.4.6.8}D$, &c.

In which series, it is pretty evident, the quantities A, B, C, D, &c. will have a convergency while y increases from 0 *ad infinitum*, although the convergency will be but slow after y becomes greater than 1. It is obvious too, that this series

vanishes together with y, and therefore needs no correction. And for this reason chiefly I have introduced it, as it affords us another mean of obtaining the value of the constant quantity d, by which the descending series are to be corrected.

11. But the fluxionary expression $\dot{y}\sqrt{\left(\frac{1+ee\,yy}{1+yy}\right)}$, obtained in Art. 1, is evidently $=\dot{y}\sqrt{\left(ee+\frac{1-ee}{1-yy}\right)}$ = also to $\dot{y}\sqrt{\left(ee-\frac{aa}{1+yy}\right)}$; and this expression converted into series, by the Binomial theorem, becomes $e\dot{y}-\frac{aa\dot{y}}{2e(1+yy)}-\frac{a^4\dot{y}}{2.4e^3(1+yy)^2}-\frac{3a^6\dot{y}}{2.4.6e^5(1+yy)^3}-\frac{3.5a^8\dot{y}}{2.4.6.8e^7(1+yy)^4}$, &c. Here again, denoting the fluents of $\frac{\dot{y}}{1+yy}$, $\frac{\dot{y}}{(1+yy)^2}$, $\frac{\dot{y}}{(1+yy)^3}$, &c. by A, B, C, &c. we shall have

$$A = \text{circ. arch, rad. 1, and tang. } y,$$

$$B = \frac{y}{2(1+yy)}+\frac{A}{2},$$

$$C = \frac{y}{4(1+yy)^2}+\frac{3B}{4},$$

$$D = \frac{y}{6(1+yy)^3}+\frac{5C}{6},$$

&c. &c.

And, by proceeding as before directed, we get (THEOREM IX.)

$$z = ey-\frac{aa}{2e}A-\frac{a^4}{2.4e^3}B-\frac{3a^6}{2.4.6e^5}C-\frac{3.5a^8}{2.4.6.8e^7}D, \text{ \&c.}$$

And this series, it is obvious, will converge the swifter the greater y is, so that it will begin to converge swiftly when the preceding series begins to converge slowly. It is evident too, that this series vanishes together with y, and therefore wants no correction. Moreover, it has an advantage over the preceding series, in that the coefficients of it decrease by the powers of $\frac{aa}{ee}$, that is, by $\frac{aa}{1+aa}$. And it supplies us with a different expression of the value of d, as will appear in the next section, to which I now proceed.

SECT. II. *The Methods of computing the Values of the constant Quantities by which the ascending Series differ from the descending ones.*

12. Now the methods of obtaining these constant quantities are such as are shewn in my *Mathematical Essays*, (published in 1788,) pages 100, 101, 102, &c. to 112; viz. either by computing the value of both an ascending and a descending series, taking for y some small definite quantity, or by comparing the values of those series together when y is taken immensely great: the former of which methods is more general, but the latter, when it can be applied, commonly affords the easiest computation. In this section, I shall make use of both these methods, as the one or the other is best suited to the case in hand. I begin with the use of the latter method, in comparing together all the different expressions of the value of z, which are reduced to few terms in the case when y becomes immensely great.

Now, when y is taken immensely great, the value of z in THEOREM III. Art. 4, becomes barely $= ey - d$. For, in this case, the H. L. $\frac{\sqrt{(ee\,yy+1)}-1}{ey}$ becomes the logarithm of the ratio of equality, which is $= 0$. And then A is barely $= \sqrt{(ee\,yy+1)} + 0 = ey + \frac{1}{2ey} - \frac{1}{2.4e^3y^3}$, &c. all which terms, after the first, vanish in this case; and therefore eeA, which occurs in the value of B, becomes barely e^2y. Moreover, the radical expression $\frac{-(ee\,yy+1)^{\frac{1}{2}}}{2yy}$, which enters into the value of B, becomes barely $= \frac{-e^2y}{2}$; and thence we have B $= \frac{-e^2y+e^2y}{2} = 0$. And, since each of the expressions $\frac{(ee\,yy+1)^{\frac{1}{2}}}{4y^4}$, $\frac{(ee\,yy+1)^{\frac{1}{2}}}{6y^6}$, &c. evidently becomes $= 0$, in this case, and since B has been

shown to be $= o$, it will thence follow, that all the terms denoted by C, D, E, &c. will vanish, and there will be left $z = ey - d$.

13. And in like manner it will appear, that the value of z in THEOREM IV. Art. 5, when y becomes immensely great, is also $= ey - d$. For, in this case, H. L. $\frac{\sqrt{(yy+1)}-1}{y}$ becomes $= o$; and each of the expressions $\frac{\sqrt{(yy+1)}}{2yy}$, $\frac{\sqrt{(yy+1)}}{4y^4}$, &c. also becomes $= o$; and, consequently, $z = \begin{cases} e\sqrt{(yy+1)} \\ -d. \end{cases}$ But, since e is a finite quantity, the expression $e\sqrt{(yy+1)} = ey + \frac{e}{2y} - \frac{e}{2.4y^3}$, &c. when y is immensely great, becomes barely $= ey$. Therefore, in this case, we have $z = ey - d$.

14. COROLLARY. And hence it appears, that the series in these two theorems are equal to each other, and, consequently, that the constant quantity to be subtracted from each of them, by way of correction, is the same.

15. The first term of the series which expresses the value of z in Theorem V, Art. 7, is $\sqrt{(uu-1)}$, which, by the notation there used, is always $= ey$. And, when y becomes immensely great, the terms $\frac{\sqrt{(uu-1)}}{2uu}$, $\frac{\sqrt{(uu-1)}}{4u^4}$, $\frac{\sqrt{(uu-1)}}{6u^6}$, &c. which enter into the values of B, C, D, &c. vanish; but A becomes $=$ *the quadrantal arch of the circle, of which the radius is* 1; and thence we have $B = \frac{A}{2}$, $C = \frac{3}{4}B$, $= \frac{3}{2.4}A$, $D = \frac{5}{6}C = \frac{3.5}{2.4.6}A$, &c. and these values being written for B, C, D, &c. in the series, we have, in this case, $z = ey - \frac{aa}{2}A + \frac{3a^4}{2.2.4}A - \frac{3.3.5a^6}{2.2.4.4.6}A + \frac{3.3.5.5.7a^8}{2.2.4.4.6.6.8}A$, &c. And, since this series always gives the correct value of z, we have now discovered the value of d, the constant quantity to be subtracted from the

descending series given in Theorems III and IV. The series to which d is $=$, viz. $A \times : \frac{aa}{2} - \frac{3a^4}{2.2.4} + \frac{3.3.5a^6}{2.2.4.4.6} - \frac{3.3.5.5.7a^8}{2.2.4.4.6.6.8}$, &c. will indeed diverge when a is greater than 1; yet, as was observed in Art. 7, it is of that form which admits of transformation into another which will always converge.

16. For the reasons above given in Articles 12 and 13, each of the terms A, B, C, &c. in Theorem VI, Art. 8, vanishes when y becomes immensely great, and z is then barely $= \begin{cases} \surd(uu + aa) \\ -d. \end{cases}$ And, since $\surd(uu + aa)$ is, by the notation in Art. 1 and 7, $= \surd(ee\,yy + ee)$, which, in this case, becomes barely $= ey$, we have $z = ey - d$. Here we see that the series in this Theorem, and in Theorems III and IV, are always $=$ to each other, and consequently differ from each of the ascending series by the same constant quantity d, the value of which was discovered in the preceding Article.

17. When y becomes immensely great, the value of z in Theorem VII, Art. 9, becomes barely $= u - d$. And, since u is universally $= \surd(ee\,yy + 1)$, it will, in this case, be $= ey$; and we shall have $z = ey - d$, which is the very expression given by all the other descending series in the like case. But, when the hyperbola is equilateral, as was supposed in Art. 9, a is $= 1$, and we have $d = 1{\cdot}57079632 \times : \frac{1}{2} - \frac{3}{2.3.4} + \frac{3.3.5}{2.2.4.4.6} - \frac{3.3.5.5.7}{2.2.4.4.6.6.8}$, &c.

Moreover, when y is $= 0$, z is also $= 0$, and u is $= 1$; and therefore, by this theorem, we have $0 = \begin{cases} 1 - \frac{1}{2.3} - \frac{3}{2.4.7} - \frac{3.5}{2.4.6.11} - \frac{3.5.7}{2.4.6.8.15}, \text{ &c. or} \\ -d \end{cases}$

$$d = 1 - \frac{1}{2.3} - \frac{3}{2.4.7} - \frac{3.5}{2.4.6.11} - \frac{3.5.7}{2.4.6.8.15}, \text{ &c.}$$

And hence it follows, that this very slowly converging series is $= 1{\cdot}57079632 \times : \frac{1}{2} - \frac{3}{2.2.4} + \frac{3.3.5}{2.2.4.4.6} - \frac{3.3.5.5.7}{2.2.4.4.6.6.8}$, &c. by which expression its value is easily attainable, and will be found to be $= 0{\cdot}59907012$.

I observe, *in transitu*, that the ratio of this slowly converging series, $1 - \frac{1}{2.3} - \frac{3}{2.4.7} - \frac{3.5}{2.4.6.11} - \frac{3.5.7}{2.4.6.8.15}$, &c. to a series of good convergency, is easily attainable; by which mean we may likewise compute its value to any degree of exactness.

18. A general expression of the value of d being found in Art. 15, by which it may be computed, whatever be the ratio of the two axes of the hyperbola, I might now proceed to show the use of the theorems by a few examples; but, as the same series is attainable another way, and the same value of d is attainable also by different series, it will be no less curious than useful to show in what manner.

19. The nth term of the series of quantities $\frac{-y^3}{2(1+yy)^2}$, $\frac{-y^5}{4(1+yy)^3}$, $\frac{-y^7}{6(1+yy)^4}$, &c. which enter into the values of B, C, D, &c. in Theorem VIII, Art. 10, is evidently $\frac{-y^{2n+1}}{2n(1+yy)^n}$, which, by the Binomial theorem, is $= \frac{-y^{2n+1}}{2n} \times : y^{-2n} - ny^{-2n-2} + n \cdot \frac{n+1}{2} y^{-2n-4} - n \cdot \frac{n+1}{2} \cdot \frac{n+2}{2} y^{-2n-6}$, &c. $= -\frac{y}{2n} + \frac{1}{2y} - \frac{n+1}{4y^3}$, &c. which, when y becomes immensely great, is barely $= \frac{y}{2n}$. And the value of A, in this case, is y — *the quadrantal arch of a circle, of which the radius is* 1. Let this quadrantal arch be denoted by α; then, by substituting for A, B, C, &c. their proper values as they thus arise, we have

$$A = \ldots\ldots\ldots\ldots\ldots\ldots\ldots y - \alpha,$$

$$B = -\frac{y}{2} + \frac{3y}{2} - \frac{3}{2}\alpha = y - \frac{3}{2}\alpha,$$

$$C = -\frac{y}{4} + \frac{5y}{4} - \frac{3\cdot5}{2\cdot4}\alpha = y - \frac{3\cdot5}{2\cdot4}\alpha,$$

$$D = -\frac{y}{6} + \frac{7y}{6} - \frac{3\cdot5\cdot7}{2\cdot4\cdot6}\alpha = y - \frac{3\cdot5\cdot7}{2\cdot4\cdot6}\alpha,$$

&c. &c.

And, lastly, by writing these values of A, B, C, &c. in the Theorem, we have, in this case,

$$z = \begin{cases} y + \frac{aa}{2}y - \frac{a^4}{2\cdot4}y + \frac{3a^6}{2\cdot4\cdot6}y - \frac{3\cdot5a^8}{2\cdot4\cdot6\cdot8}y, & \text{&c.} \\ -\frac{aa}{2}\alpha + \frac{3a^4}{2\cdot2\cdot4}\alpha - \frac{3\cdot5a^6}{2\cdot2\cdot4\cdot4\cdot6}\alpha + \frac{3\cdot5\cdot5\cdot7a^8}{2\cdot2\cdot4\cdot4\cdot6\cdot6\cdot8}\alpha, & \text{&c.} \end{cases}$$

But the series $y + \frac{aa}{2}y - \frac{a^4}{2\cdot4}y + \frac{3a^6}{2\cdot4\cdot6}y - \frac{3\cdot5a^8}{2\cdot4\cdot6\cdot8}y$, &c. is evidently $= y\sqrt{(1 + aa)}$, which, by the notation in Art. 1, is $= ey$. We therefore have, in this case,

$$z = ey - \alpha \times : \frac{aa}{2} - \frac{3a^4}{2\cdot2\cdot4} + \frac{3\cdot5a^6}{2\cdot2\cdot4\cdot4\cdot6} - \frac{3\cdot5\cdot5\cdot7a^8}{2\cdot2\cdot4\cdot4\cdot6\cdot6\cdot8}, \text{ &c.}$$

And, since this theorem always gives the correct value of z, we have now the satisfaction of seeing a confirmation of the truth of our conclusion in Art. 15, by obtaining the very same expression by a very different process.

20. From what has been shewn in Articles 12, 13, &c. it will be very evident to any one who runs his eye over the component parts of the series given in Theorem IX, Art. 11, that, when y becomes immensely great, A becomes $=$ *the quadrantal arch of a circle, of which the radius is* 1, which arch was denoted in the preceding Art. by α; and that

$$B = 0 + \frac{A}{2} = \frac{1}{2}\alpha,$$

$$C = 0 + \frac{3B}{4} = \frac{3}{2\cdot4}\alpha,$$

$$D = 0 + \frac{5C}{6} = \frac{3\cdot5}{2\cdot4\cdot6}\alpha,$$

&c. &c.

And, these values being written for A, B, C, &c. in the Theorem, it gives, in this case,

$$z = ey - \frac{aa}{2e}\alpha - \frac{a^4}{2.2.4e^3}\alpha - \frac{3.3a^6}{2.2.4.4e^5}\alpha - \frac{3.3.5.5a^8}{2.2.4.4.6.6.8e^7}\alpha, \text{ \&c.}$$

And, since this Theorem also always gives the correct value of z, we shall, by comparing the expression now obtained with those which were found for z, in the like case, in Articles 12, 13, 15, 16, and 19, see that we have now got another general expression of the value of d, viz. $\alpha \times : \frac{aa}{2e} + \frac{a^4}{2.2.4e^3} + \frac{3.3a^6}{2.2.4.4.6e^5} + \frac{3.3.5.5a^8}{2.2.4.4.6.6.8e^7}$, &c. in which series ee is $= aa + 1$, and therefore it must always converge. Yet it should not be hastily concluded, that this expression of the value of d is always preferable to that which was obtained in Articles 15 and 19; for, when a is a large number, the powers of $\frac{aa}{ee} = \frac{aa}{1+aa}$, by which the series converges, will decrease very slowly.

21. However, when it happens that a is a large number, the value of d may be obtained by means of two series, which, in that case, will converge pretty swiftly; or indeed by means of three series, each of which will converge about twice as fast as either of the two series. But, for the sake of brevity, I shall at present describe the method of computing the value of d by two series only, and so conclude this section.

The series proper to be used on this occasion, it is obvious, are those which are given in Theorems II and IV, Articles 3 and 5; and the value of y to be assumed, is $\frac{1}{\sqrt{e}}$, with which value each of the series will have nearly the same rate of convergency. As this will best appear by an example, I will give one, taking $a = 7$. Now, with this value of a, we have $ee = aa + 1 = 50$, and $y = \frac{1}{\sqrt{e}} = \frac{1}{\sqrt{50}} = 0{\cdot}141421356$; and, by

writing these values for e and y in Theorem II, Art. 3, we have

$$A = \frac{1}{2\sqrt{e}}\sqrt{(1+e)} + \frac{1}{2e}\text{H.L.}\left(\sqrt{e} + \sqrt{(1+e)}\right) = 0{\cdot}6547{,}820,$$

$$B = \frac{e^{-\frac{1}{2}}(1+e)^{\frac{1}{2}} - A}{4e} = 0{\cdot}0398{,}409,$$

$$C = \frac{e^{-\frac{1}{2}}(1+e)^{\frac{1}{2}} - 3B}{6e} = 0{\cdot}0036{,}665,$$

$$D = \frac{e^{-\frac{1}{2}}(1+e)^{\frac{1}{2}} - 5C}{8e} = 0{\cdot}0003{,}853.$$

$$E = \frac{e^{-\frac{1}{2}}(1+e)^{\frac{1}{2}} - 7D}{10e} = 0{\cdot}0000{,}484,$$

$$F = \frac{e^{-\frac{1}{2}}(1+e)^{\frac{1}{2}} - 9E}{12e} = 0{\cdot}0000{,}051,$$

$$G = \frac{e^{-\frac{1}{2}}(1+e)^{\frac{1}{2}} - 11F}{14e} = 0{\cdot}0000{,}006,$$

&c. &c.

and thence

+	−
$A = 0{\cdot}6547{,}820$	$\frac{1}{2}B = 0{\cdot}0199{,}205$
$\frac{3}{2.4}C = 0{\cdot}0018{,}750$	$\frac{3.5}{2.4.6}D = 0{\cdot}0001{,}204$
$\frac{3.5.7}{2.4.6.8}E = 0{\cdot}0000{,}119$	$\frac{3.5.7.9}{2.4.6.8.10}F = 0{\cdot}0000{,}013$
$\frac{3.5.7.9.11}{2.4.6.8.10.12}G = 0{\cdot}0000{,}001$	
$+ 0{\cdot}6561{,}190$	$- 0{\cdot}0200{,}422$
$- 0{\cdot}0200{,}422$	

and $z = 0{\cdot}6360{,}768$; which value of z needs no correction.

We must now, in order to find the value of d, write the same values of e and y in Theorem IV, Art. 5, where we shall then have

$$A = \text{H. L.}\,(\sqrt{(1+e)} - \sqrt{e}),$$

$$B = \frac{-e\sqrt{\left(\frac{1}{e}+1\right)} - A}{2},$$

$$C = \frac{-ee\sqrt{\left(\frac{1}{e}+1\right)} - 3B}{4},$$

$$D = \frac{-e^3\sqrt{\left(\frac{1}{e}+1\right)} - 5C}{6},$$

$$E = \frac{-e^4\sqrt{\left(\frac{1}{e}+1\right)} - 7D}{8},$$

$$F = \frac{-e^5\sqrt{\left(\frac{1}{e}+1\right)} - 9E}{10},$$

&c. &c.

But, since the terms A, B, C, &c. are to be divided by e, e^2, e^3, &c. respectively, it will be best to divide them by these quantities, before we begin the arithmetical calculations; otherwise much unnecessary labour must be taken. The terms, then, being so divided, and the proper value of e being written for it, viz. $\sqrt{50}$, we shall have as below:

$$\frac{A}{e} = \frac{\text{H. L.}}{e}\left(\sqrt{(1+e)} - \sqrt{e}\right) = -0.2410,905,$$

$$\frac{B}{e^2} = \frac{-\sqrt{\left(\frac{1}{e}+1\right)}}{2ee} - \frac{A}{2e^2} = -0.0082,728,$$

$$\frac{C}{e^3} = \frac{-\sqrt{\left(\frac{1}{e}+1\right)}}{4e^3} - \frac{3B}{4e^3} = -0.0006,314,$$

$$\frac{D}{e^4} = \frac{-\sqrt{\left(\frac{1}{e}+1\right)}}{6e^4} - \frac{5C}{6e^4} = -0.0000,607,$$

$$\frac{E}{e^5} = \frac{-\sqrt{\left(\frac{1}{e}+1\right)}}{8e^5} - \frac{7D}{8e^5} = -0.0000,065,$$

$$\frac{F}{e^{11}} = \frac{-\sqrt{\left(\frac{1}{e}+1\right)}}{10e^6} - \frac{9E}{10e^{11}} = -0.0000,007,$$

&c. &c.

And thence

+		−	
$e\sqrt{\left(\frac{1}{e}+1\right)}$	$= 7{\cdot}5545{,}396,$	$+\frac{A}{2e}$	$= 0{\cdot}1205{,}452,$
$-\frac{B}{2.4e^3}$	$= 0{\cdot}0010{,}341,$	$+\frac{3C}{2.4.6e^5}$	$= 0{\cdot}0000{,}395$
$-\frac{3.5D}{2.4.6.8e^7}$	$= 0{\cdot}0000{,}024,$	$+\frac{3.5.7E}{2.4.6.8.10e^9}$	$= 0{\cdot}0000{,}002$
	$+ 7{\cdot}5555{,}761$		$- 0{\cdot}1205{,}849$
	$- 0{\cdot}1205{,}849$		

and $z = 7{\cdot}4349{,}912 - d$.

But, by the foregoing part of this article, $z = 0{\cdot}6360{,}768$; we therefore have $d = 7{\cdot}4349{,}912 - 0{\cdot}6360{,}768 = 6{\cdot}7989{,}144$.

22. With the value of a above given, viz. 7, we see a swift convergency, both in the ascending and in the descending series; but, if a were given $= \sqrt{3}$, (which is as small a value of a as need be used in these theorems, for this purpose, because if it were less than, or even $= \sqrt{3}$, the value of d might be computed by one series only, as was observed in Art. 15,) each of the series would converge but slowly, $\frac{1}{e}$, in this case, being $= \frac{1}{2}$; to remedy which, as the terms of each of the series have the signs + and − alternately, it would be expedient to compute a moderate number (from six to ten, as the case shall require,) of the initial terms of each, and then to transform the remainders into other series, which should converge by the powers of $\frac{1}{4+e}$, instead of the powers of $\frac{1}{e}$. This increase of convergency in the geometrical progression, assisted as it would be by the decrease of the coefficients of the new series, would enable us to get a result accurate enough for all common uses, by computing ten (or fewer) terms of each of the new series.

But, as the transformation now mentioned requires but a moderate skill in series, I shall, for the sake of brevity, omit examples of it, and proceed to

Sect. III. *Examples of the Use of the foregoing Theorems.*

28. My intention in this section is, to illustrate the use of the foregoing theorems by a few examples, selecting at the same time such of the theorems as are best adapted to the case in hand; by which, and attention to what was said in the first section, of the limits of the convergency of the several series, I hope the reader will be directed how to make a proper choice of theorems on all other occasions.

Example 1.

Let there be an hyperbola of which the semi-axes are 40 and 30 respectively, and the ordinate is 10; it is required to find the length of the arch from the vertex of the ordinate.

Since the conjugate semi-axis of this hyperbola is 30, we must, in order to fit the given numbers to our theorems, divide them all by 30; and then we shall have the corresponding dimensions of a similar hyperbola as follows; viz. the transverse semi-axis $= \frac{4}{3}$, the conjugate semi-axis $= 1$, and the ordinate $= \frac{1}{3}$. And the proper theorem to be used in this case is the second.

Writing, then, $\frac{4}{3}$ for a, and $\frac{1}{3}$ for y, in the IId Theorem, we have $ee = aa + 1 = \frac{25}{9}$, and

$A = \frac{y}{2}\sqrt{(1+eeyy)} + \frac{1}{2e}\text{H.L.}\,(ey + \sqrt{(1+eeyy)}) = 0{\cdot}3497{,}6260,$

$B = \frac{y(1+eeyy)^{\frac{1}{2}} - A}{4ee} = 0{\cdot}0134{,}3234,$

$C = \frac{y^3(1+eeyy)^{\frac{1}{2}} - 3B}{6ee} = 0{\cdot}0009{,}0892,$

$D = \frac{y^5(1+eeyy)^{\frac{1}{2}} - 5C}{8ee} = 0{\cdot}0000{,}7272,$

$E = \frac{y^7(1+eeyy)^{\frac{1}{2}} - 7D}{10ee} = 0{\cdot}0000{,}0632,$

$F = \frac{y^9(1+eeyy)^{\frac{1}{2}} - 9E}{12ee} = 0{\cdot}0000{,}0058,$

$G = \frac{y^{11}(1+eeyy)^{\frac{1}{2}} - 11F}{14ee} = 0{\cdot}0000{,}0005;$

and thence

+	−
$A = 0{\cdot}3497{,}6260,$	$\frac{1}{2}B = 0{\cdot}0067{,}1617,$
$\frac{3}{2.4}C = 0{\cdot}0003{,}4084,$	$\frac{3.5}{2.4.6}D = 0{\cdot}0000{,}2273,$
$\frac{3.5.7}{2.4.6.8}E = 0{\cdot}0000{,}0173,$	$\frac{3.5.7.9}{2.4.6.8.10}F = 0{\cdot}0000{,}0014,$
$\frac{3.5.7.9.11}{2.4.6.8.10.12}G = 0{\cdot}0000{,}0001,$	
and the sum $= + 0{\cdot}3501{,}0518,$	and the sum $= - 0{\cdot}0067{,}3904,$
$- 0{\cdot}0067{,}3904,$	

difference of these sums is $0{\cdot}3433{,}6614 = z$, the length of the arch of an hyperbola, from the vertex to the ordinate, of which the transverse and conjugate semi-axes are $\frac{4}{3}$ and 1, and the ordinate $\frac{1}{3}$. And, since like parts of similar hyperbolas are to each other as their semi-axes, we shall have, by multiplying $0{\cdot}3433{,}6614$ by 30, the semi-conjugate of the hyperbola proposed, $10{\cdot}3009{,}842$ for the length required.

Having in this example shown how to adapt these theorems to hyperbolas that have a conjugate semi-axis different from 1,

it need not be repeated again. I shall therefore, in the remaining examples, show the convergency of these new series in most of the different cases that can occur.

Example II.

24. Given $a = 1$, and $y = 1$, to find z.

This arch, it is obvious, may be computed by Theorem IVth, VIth, VIIth, and some others; but the VIIth is the proper one to be chosen on this occasion, as the series there given has the swifter convergency.

Writing, then, 1 for a, and 1 for y, in Theorem VII, we have (by Article 1,) $ee = aa + 1 = 2$, and, (by Art. 7,) $uu = 1 + ee\,yy = 3$; and then, (by Art. 9,)

$$u = \sqrt{3} = \begin{array}{r} + \\ 1{\cdot}7320{,}5081, \\ \hline - \end{array}$$

$$\frac{1}{2.3}u^{-3} = 0{\cdot}0320{,}7501,$$

$$\frac{3}{2.4.7}u^{-7} = 0{\cdot}0011{,}4554,$$

$$\frac{3.5}{2.4.6.11}u^{-11} = 0{\cdot}0000{,}6750,$$

$$\frac{3.5.7}{2.4.6.8.15}u^{-15} = 0{\cdot}0000{,}0481,$$

$$\frac{3.5.7.9}{2.4.6.8.10.19}u^{-19} = 0{\cdot}0000{,}0038,$$

$$\frac{3.5.7.9.11}{2.4.6.8.10.12.23}u^{-23} = 0{\cdot}0000{,}0003;$$

sum of the neg. terms $-\ 0{\cdot}0332{,}9327;$

sum of the series $+\ 1{\cdot}6987{,}5754;$

correction of the fluent $-\ 0{\cdot}5990{,}7012 = -d$, (by Art. 17;)

the difference of which is $+\ 1{\cdot}0996{,}8742 = z$.

EXAMPLE III.

25. Given $a = 1$, and $y = \sqrt{(1000000 - 1)} = 999{\cdot}9995$ nearly, to find x.

This arch, it is very obvious, may be computed by Theorem IIId, IVth, VIth, and VIIth, the series in each of them converging, in this case, very swiftly. And it may be computed also by the IXth; but the proper Theorem to be used in this case is the VIIth.

Now, since ee is $= 2$, and $y = \sqrt{(1000000 - 1)}$, we have (by Article 7,) $u = \sqrt{(2yy + 1)} = \sqrt{(2000000 - 1)} = \sqrt{2} \times \sqrt{(1000000 - \frac{1}{2})} = \sqrt{2} \times \left(1000 - \frac{1}{4.1000}\right)$ very nearly, $= 1000\sqrt{2} - \frac{\sqrt{2}}{4000} = 1414{\cdot}21320\,88$, which may be taken for the value of the whole series, since $\frac{1}{2}u^{-3}$, the second term of it, does not give a 1 in the tenth place of decimals. If, therefore, from $u = 1414{\cdot}21320\,88$, we subtract $d = 0{\cdot}5990701$, (by Article 17,) we shall have $x = 1413{\cdot}6141387$,* the length required.

EXAMPLE IV.

26. Let a be given $= 7$, and $y = 10$, to find x.

This Example may be computed by Theorem IIId, IVth, VIth, and some others; the VIth is to be chosen rather than the IIId, and the IVth rather than the VIth.

* The computation of the value of x, in this example, is the problem alluded to in the Introduction to this Paper, which first turned my thoughts to the subject of it, in the year 1770. In the next year, two answers were given to it, by two persons of good reputation for their skill in mathematics, one of them making $x = 1414{\cdot}2132088$, the other, $x = 1413{\cdot}8921$. These two are the only solutions of this problem that I know of; and, if my calculation be right, both are erroneous.

Now, if 10 be written for y in Theorem IVth, we shall have

$$A = \text{H. L.}\frac{\sqrt{(yy+1)}-1}{y} = -0{\cdot}0998{,}341,$$

$$B = \frac{-\sqrt{(yy+1)}}{2yy} - \frac{A}{2} = -0{\cdot}0003{,}323,$$

$$C = \frac{-\sqrt{(yy+1)}}{4y^4} - \frac{3B}{4} = -0{\cdot}0000{,}020;$$

of which terms, two only are wanted to obtain a result true to seven places of figures. And then, ee being $= aa + 1 = 50$, we have

	+	−
$e\sqrt{(yy+1)} = 71{\cdot}0638{,}520,$	$+\frac{1}{2e}A = 0{\cdot}0070{,}593,$	
$-\frac{1}{2.4e^3}B = 0{\cdot}0000{,}001,$		$-d = 6{\cdot}7989{,}144,$ (Art. 21,)
sum of the posit. terms	$71{\cdot}0633{,}521;$	the sum $-6{\cdot}8059{,}737;$
neg. term, and correct.		$-6{\cdot}8059{,}737;$

the difference is $+64{\cdot}2573{,}748 = x$.

Example v.

27. Let a be given $= \frac{1}{2}$, and $y = 10$, to find x.

This example may be computed by Theorem IIId, IVth, Vth, VIth, VIIIth, and IXth; of which the IVth, Vth, and IXth, are more eligible than the other three. I make choice of the fourth, on account of the facility of the computation by it, with the present value of y.

Now, by writing 10 for y in Theorem IV, we shall have (as in the preceding example,)

$$A = \text{H. L.}\frac{\sqrt{(yy+1)}-1}{y} = -0{\cdot}0998{,}341,$$

$$B = \frac{-\sqrt{(yy+1)}}{2yy} - \frac{A}{2} = -0{\cdot}0003{,}323,$$

$$C = \frac{-\sqrt{(yy+1)}}{4y^4} - \frac{3B}{4} = -0{\cdot}0000{,}020;$$

and then, ee being $= aa + 1 = \frac{5}{4}$, we have

+	−
$e\sqrt{(yy+1)} = 11{\cdot}2361{,}025$,	$\frac{A}{2e} = 0{\cdot}0446{,}472$,
$-\frac{1}{2\,4^1}B = 0{\cdot}0000{,}297$,	$\frac{3}{2\cdot 4\,6^2}C = 0{\cdot}0000{,}001$,
	$-d = 0{\cdot}1803{,}793$. (Art. 15.)
sum of affirm. terms $+ 11{\cdot}2361{,}322$;	
neg. terms and corr. $- 0{\cdot}2250{,}266$;	the sum is $- 0{\cdot}2250{,}266$.
the difference is $= 11{\cdot}0111{,}056$,	

which is $= z$.

28. Having now produced series, of good convergency, for computing the length of the arch from the vertex to the ordinate, (and consequently any portion of such an arch,) of any conical hyperbola, I shall conclude this Paper with a few remarks: reserving some other theorems which I have discovered for the purpose, till I shall have found an opportunity to describe nearly an equal number of theorems, which I have long had by me, for the Rectification of the Ellipsis.

The utility of hyperbolic and elliptic arches, in the solution of various problems, and particularly in the business of computing fluents, has been shown by those eminent mathematicians, M^c^LAURIN, SIMPSON, and LANDEN; the last of whom hath written a very ingenious paper on hyperbolic and elliptic arches, which was published in the Ist volume of his *Mathematical Memoirs*, in the year 1780. I have indeed heard, that some improvement in the rectification of the ellipsis and hyperbola had been produced, and some of the same theorems discovered, by a learned Italian, many years before Mr. LANDEN's *Mathematical Memoirs* were published; but, as Mr. LANDEN has declared that he had never seen nor heard any thing of that work, and as various instances are to be found of different men

discovering the same truth, without any knowledge of each other's works, I see no reason for disbelieving him. But I have seen no writings on this subject which contain any thing more than what is very common, besides those of the three gentlemen above mentioned, and Dr. WARING's *Meditationes Analyticæ*; and, while I have no inclination to detract from their merits, I may be allowed to say that I have borrowed nothing from their works.

29. With respect to Dr. WARING, (who was well known to be a profound mathematician, and I can testify that he was a good-natured man,) he has given, in page 470 of his *Meditationes Analyticæ*, (published in 1776,) these two series, as expressions of the length of an arch of an equilateral hyperbola; viz.

"Arcus hyperbolicus exprimi possit per seriem $-\frac{1}{x}+\frac{1}{2\times 3}x^{3}$ " $-\frac{1}{2^{3}.2\times 7}x^{7}+\frac{1.3}{2^{7}.2.3\times 11}x^{11}-\frac{1.3.5}{2^{9}.2.3.4\times 15}x^{15}+\frac{1.3.5.7}{2^{11}.2.3.4.5\times 19}x^{19}$, " &c. ubi x denotat abscissam ad asymptoton."

"Si vero requiratur descendens series, tum erit $x-\frac{1}{2\times 3}x^{-3}$ " $+\frac{1}{2^{3}.2\times 7}x^{-7}-\frac{3^{*}}{2^{5}.2.3\times 11}x^{-11}$, &c. quæ, quoad coefficientes " attinet, prorsus eandem observat legem ac præcedens."

30. These series, as they now stand, are of little use. But, if proper corrections were applied to them, (which may easily be done from what has been shewn in this Paper, and in my *Mathematical Essays*,) and the first of them were transformed into another series converging by the powers of $\frac{x^{4}}{1+x^{4}}$, they would become very useful for computing any arch of an equilateral

* In the original, this term is erroneously printed, there being a 1 in the numerator, instead of a 3.

hyperbola, when the abscissa is taken on the asymptote. This I thought it might be proper to remark, that the less experienced readers of this Paper might not be misled by so great an authority as that of Dr. WARING. Whether or not he ever corrected these oversights in any of his subsequent publications, I cannot ascertain, for want of books.

XVIII. *Catalogue of 500 new Nebulæ, nebulous Stars, planetary Nebulæ, and Clusters of Stars; with Remarks on the Construction of the Heavens.* By William Herschel, *LL. D. F. R. S.*

Read July 1, 1802.

Since the publication of my former two catalogues of nebulæ, I have, in the continuation of my telescopic sweeps, met with a number of objects that will enrich our natural history, as it may be called, of the heavens. A catalogue of them will be found at the end of this paper, containing 500 new nebulæ, nebulous stars, planetary nebulæ, and clusters of stars. These objects have been arranged in eight classes, in conformity with the former catalogues, of which the present one is therefore a regular continuation. This renders it unnecessary to give any further explanation, either of the contents of its columns, or the abbreviations which have been used in the description of the objects.

It has hitherto been the chief employment of the physical astronomer, to search for new celestial objects, whatsoever might be their nature or condition; but our stock of materials is now so increased, that we should begin to arrange them more scientifically. The classification adopted in my catalogues, is little more than an arrangement of the objects for the convenience of the observer, and may be compared to the disposition of the books in a library, where the different sizes of the volumes is

often more considered than their contents. But here, in dividing the different parts of which the sidereal heavens are composed into proper classes, I shall have to examine the nature of the various celestial objects that have been hitherto discovered, in order to arrange them in a manner most conformable to their construction. This will bring on some extensive considerations, which would be too long for the compass of a single paper; I shall therefore now only give an enumeration of the species that offer themselves already to our view, and leave a particular examination of the separate divisions, for some early future occasions.

In proceeding from the most simple to the more complex arrangements, several methods, taken from the known laws of gravitation, will be suggested, by which the various systems under consideration may be maintained; but here also we shall confine ourselves to a general review of the subject, as observation must furnish us first with the necessary data, to establish the application of any one of these methods on a proper foundation.

ENUMERATION OF THE PARTS THAT ENTER INTO THE CONSTRUCTION OF THE HEAVENS.

I. *Of insulated Stars.*

In beginning our proposed enumeration, it might be expected that the solar system would stand foremost in the list; whereas, by treating of insulated stars, we seem, as it were, to overlook one of the great component parts of the universe. It will, however, soon appear that this very system, magnificent as it is, can only rank as a single individual belonging to the species which we are going to consider.

By calling a star insulated, I do not mean to denote its being totally unconnected with all other stars or systems; for no one, by the laws of gravitation, can be intirely free from the influence of other celestial bodies. But, when stars are situated at such immense distances from each other as our sun, Arcturus, Capella, Lyra, Sirius, Canobus, Markab, Bellatrix, Menkar, Shedir, Algorah, Propus, and numberless others probably are, we may then look upon them as sufficiently out of the reach of mutual attractions, to deserve the name of insulated stars.

In order not to take this assertion for granted, without some examination, let us admit, as is highly probable, that the whole orbit of the earth's annual motion does not subtend more than an angle of one second of a degree, when seen from Sirius. In consequence of this, it appears by computation, that our sun and Sirius, if we suppose their masses to be equal, would not fall together in less than 39 millions of years, even though they were not impeded by many contrary attractions of other neighbouring insulated stars; and that, consequently, with the assistance of the opposite energies exerted by such surrounding stars, these two bodies may remain for millions of ages, in a state almost equal to undisturbed rest. A star thus situated may certainly deserve to be called insulated, since it does not immediately enter into connection with any neighbouring star; and it is therefore highly probable, that our sun is one of a great number that are in similar circumstances. To this may be added, that the stars we consider as insulated are also surrounded by a magnificent collection of innumerable stars, called the milky-way, which must occasion a very powerful balance of opposite attractions, to hold the intermediate stars in a state of rest. For, though our sun, and all the stars we see, may

truly be said to be in the plane of the milky-way, yet I am now convinced, by a long inspection and continued examination of it, that the milky-way itself consists of stars very differently scattered from those which are immediately about us. But of this, more will be said on another occasion.

From the detached situation of insulated stars, it appears that they are capable of being the centres of extensive planetary systems. Of this we have a convincing proof in our sun, which, according to our classification, is one of these stars. Now, as we enjoy the advantage of being able to view the solar system in all its parts, by means of our telescopes, and are therefore sufficiently acquainted with it, there will be no occasion to enter into a detail of its construction.

The question will now arise, whether every insulated star be a sun like ours, attended with planets, satellites, and numerous comets? And here, as nothing appears against the supposition, we may from analogy admit the probability of it. But, were we to extend this argument to other sidereal constructions, or, still farther, to every star of the heavens, as has been done frequently, I should not only hesitate, but even think that, from what will be said of stars which enter into complicated sidereal systems, the contrary is far more likely to be the case; and that, probably, we can only look for solar systems among insulated stars.

II. *Of Binary sidereal Systems, or double Stars.*

The next part in the construction of the heavens, that offers itself to our consideration, is the union of two stars, that are formed together into one system, by the laws of attraction.

If a certain star should be situated at any, perhaps immense,

distance behind another, and but very little deviating from the line in which we see the first, we should then have the appearance of a double star. But these stars, being totally unconnected, would not form a binary system. If, on the contrary, two stars should really be situated very near each other, and at the same time so far insulated as not to be materially affected by the attractions of neighbouring stars, they will then compose a separate system, and remain united by the bond of their own mutual gravitation towards each other. This should be called a real double star; and any two stars that are thus mutually connected, form the binary sidereal system which we are now to consider.

It is easy to prove, from the doctrine of gravitation, that two stars may be so connected together as to perform circles, or similar ellipses, round their common centre of gravity. In this case, they will always move in directions opposite and parallel to each other; and their system, if not destroyed by some foreign cause, will remain permanent.

Figure 1 (Plate XVI.) represents two equal stars *a* and *b*, moving in one common circular orbit round the centre *o*, but in the opposite directions of *at* and *bt*. In Fig. 2 we have a similar connection of the two stars *a b*; but, as they are of different magnitudes, or contain unequal quantities of matter, they will move in circular orbits of different dimensions round their common centre of gravity *o*. Fig. 3 represents equal, and Fig. 4 unequal stars, moving in similar elliptical orbits round a common centre; and, in all these cases, the directions of the tangents *t t*, in the places *a b*, where the stars are, will be opposite and parallel, as will be more fully explained hereafter.

These four orbits, simple as they are, open an extensive field

for reflection, and, I may add, for calculation. They shew, even before we come to more complicated combinations, where the same will be confirmed, that there is an essential difference between the construction of solar and sidereal systems. In each solar system, we have a very ponderous attractive centre, by which all the planets, satellites, and comets are governed, and kept in their orbits. Sidereal systems take a greater scope: the stars of which they are composed move round an empty centre, to which they are nevertheless as firmly bound as the planets to their massy one. It is however not necessary here to enlarge on distinctions which will hereafter be strongly supported by facts, when clusters of stars come to be considered. I shall only add, that in the subordinate bodies of the solar system itself, we have already instances, in miniature, as it may be called, of the principle whereby the laws of attraction are applicable to the solution of the most complicated phenomena of the heavens, by means of revolutions round empty centres. For, although both the earth and its moon are retained in their orbits by the sun, yet their mutual subordinate system is such, that they perform secondary monthly revolutions round a centre without a body placed in it. The same indeed, though under very narrow limits, may be said of the sun and each planet itself.

That no insulated stars, of nearly an equal size and distance, can appear double to us, may be proved thus. Let Arcturus and Lyra be the stars: these, by the rule of insulation, which we must now suppose can only take place when their distance from each other is not less than that of Sirius from us, if very accurately placed, would be seen under an angle of 60 degrees from each other. They really are at about 59°. Now, in order to

make these stars appear to us near enough to come under the denomination of a double star of the first class, we should remove the earth from them at least 41253 times farther than Sirius is from us. But the space-penetrating power of a 7-feet reflector, by which my observations on double stars have been made, cannot intitle us to see stars at such an immense distance; for, even the 40-feet telescope, as has been shewn,* can only reach stars of the 1342d magnitude. It follows, therefore, that these stars could not remain visible in a 7-feet reflector, if they were so far removed as to make their angular distance less than about 94¼ minutes; nor could even the 40-feet telescope, under the same circumstances of removal, shew them, unless they were to be seen at least 9¼ minutes asunder. Moreover, this calculation is made on a supposition that the stars of which a double star is composed, might be as small as any that can possibly be perceived; but if, on the contrary, they should still appear of a considerable size, it will then be so much the more evident that such stars cannot have any great real distance, and that, consequently, insulated stars cannot appear double, if they are situated at equal distances from us. If, however, their arrangement should be such as has been mentioned before, then, one of them being far behind the other, an apparent double star may certainly be produced; but here the appearance of proximity would be deceptive; and the object so circumstanced could not be classed in the list of binary systems. However, as we must grant, that in particular situations stars apparently double may be composed of such as are insulated, it cannot be improper to consult calculation, in order to see whether it be likely that the 700 double stars I have given in two catalogues, as well as

* See Phil. Trans. for 1800, Part I. page 83.

many more I have since collected, should be of that kind. Such an inquiry, though not very material to our present purpose, will hereafter be of use to us, when we come to consider more complicated systems. For, if it can be shown that the odds are very much against the casual production of double stars, the same argument will be still more forcible, when applied to treble, quadruple, or multiple compositions.

Let us take ζ Aquarii, for an instance of computation. This star is admitted, by FLAMSTEED, DE LA CAILLE, BRADLEY, and MAYER, to be of the 4th magnitude. The two stars that compose it being equal in brightness, each of them may be supposed to shine with half the light of the whole lustre. This, according to our way of reckoning magnitudes,* would make them 4m $\times \sqrt{2} = 5\frac{3}{7}$m; that is, of between the 6th and 5th magnitude each. Now, the light we receive from a star being as the square of its diameter directly, and as the square of its distance inversely, if one of the stars of ζ Aquarii be farther off than the stars of between the 6th and 5th magnitude are from us, it must be so much larger in diameter, in order to give us an equal quantity of light. Let it be at the distance of the stars of the 7th magnitude; then its diameter will be to the diameter of the star which is nearest to us as 7 to $5\frac{3}{7}$, and its bulk as 1,885 to 1; which is almost double that of the nearest star. Then, putting the number of stars we call of between the 6th and 5th magnitude at 450, we shall have 686 of the 7th magnitude to combine with them, so that they may make up a double star of the first class, that is to say, that the two stars may not be more than 5″ asunder. The surface of the globe contains

* The expressions 2m, 3m, 4m, &c. stand for stars at the distance of 2, 3, 4, &c. times that of Sirius, supposed unity.

34036131547 circular spaces, each of 5″ in diameter; so that each of the 686 stars will have 49615357 of these circles in which it might be placed; but, of all that number, a single one would only be the proper situation in which it could make up a double star with one of the 450 given stars. But these odds, which are above 75½ millions to one against the composition of ζ Aquarii, are extremely increased by our foregoing calculation of the required size of the star, which must contain nearly double the mass allotted to other stars of the 7th magnitude; of which, therefore, none but this one can be proper for making up the required double star. If the stars of the 8th and 9th magnitudes, of which there will be 896 and 1134, should be taken in, by way of increasing the chance in favour of the supposed composition of our double star, the advantage intended to be obtained by the addition of numbers, will be completely counteracted by the requisite uncommon bulk of the star which is to serve the purpose; for, one of the 8th magnitude, ought to be more than 2¼ times bigger than the rest; and, if the composition were made by a star of the 9th magnitude, no less than four times the bulk of the other star which is to enter the composition of the double star would answer the purpose of its required brightness. Hence therefore it is evident, that casual situations will not account for the multiplied phenomena of double stars, and that consequently their existence must be owing to the influence of some general law of nature; now, as the mutual gravitation of bodies towards each other is quite sufficient to account for the union of two stars, we are authorised to ascribe such combinations to that principle.

It will not be necessary to insist any further on arguments drawn from calculation, as I shall soon communicate a series of

observations made on double stars, whereby it will be seen, that *many of them have actually changed their situation with regard to each other, in a progressive course, denoting a periodical revolution round each other; and that the motion of some of them is direct, while that of others is retrograde.* Should these observations be found sufficiently conclusive, we may already have their periodical times near enough to calculate, within a certain degree of approximation, the parallax and mutual distance of the stars which compose these systems, by measuring their orbits, which subtend a visible angle.

Before we leave the subject of binary systems, I should remark, that it evidently appears, that our sun does not enter into a combination with any other star, so as to form one of these systems with it. This could not take place without our immediately perceiving it; and, though we may have good reason to believe that our system is not perfectly at rest, yet the causes of its proper motion are more probably to be ascribed to some perturbations arising from the proper motion of neighbouring stars or systems, than to be placed to the account of a periodical revolution round some imaginary distant centre.

III. *Of more complicated sidereal Systems, or treble, quadruple, quintuple, and multiple Stars.*

Those who have admitted our arguments for the existence of real double stars, will easily advance a step farther, and allow that three stars may be connected in one mutual system of reciprocal attraction. And, as we have from theory pointed out, in figures 1, 2, 3, and 4, how two stars may be maintained in a binary system, we shall here shew that three stars may

likewise be preserved in a permanent connection, by revolving in proper orbits about a common centre of motion.

In all cases where stars are supposed to move round an empty centre, in equal periodical times, it may be proved that an imaginary attractive force may be supposed to be lodged in that centre, which increases in a direct ratio of the distances. For since, in different circles, by the law of centripetal forces, the squares of the periodical times are as the radii divided by the central attractive forces, it follows, that when these periodical times are equal, the forces will be as the radii. Hence we conclude, that in any system of bodies, where the attractive forces of all the rest upon any one of them, when reduced to a direction as coming from the empty centre, can be shewn to be in a direct ratio of the distance of that body from the centre, the system may revolve together without perturbation, and remain permanently connected without a central body.

Hence may be proved, as has been mentioned before, that two stars will move round a hypothetical centre of attraction. For, let it be supposed that the empty centre o, in Fig. 1 and 3, is possessed of an attractive force, increasing in the direct ratio of the distances $oa : ob$. Then, since here ao and bo are equal, the hypothetical attractions will be equal, and the bodies will revolve in equal times. That this agrees with the general law of attraction, is proved thus. The real attraction of b upon a is $\frac{b}{ab^2}$; and that of a upon b is $\frac{a}{ab^2}$; and, since $b = a$, it will be $\frac{b}{ab^2} : \frac{a}{ab^2} :: ao : bo$; which was required.

In Figures 2 and 4, when the stars a and b are unequal, and their distances from o also unequal, let $oa = n$, and $ob = m$; and let the mass of matter in $a = m$, and in $b = n$. Then the

attraction of b on $a = \frac{b}{ab^2}$, will be to the attraction of a on $b = \frac{a}{ab^2}$, as $n : m$; which is again directly as $ao : bo$.

I proceed now to explain a combination of three bodies, moving round a centre of hypothetical attraction. Fig. 5 contains a single orbit, wherein three equal bodies $a\ b\ c$, placed at equal distances, may revolve permanently. For, the real attraction of b on a will be expressed by $\frac{a}{ab^2}$; but this, reduced to the direction ao, will be only $\frac{b \cdot by}{ab^3}$; for, the attraction in the direction ba is to that in the direction by, parallel to ao, as $\frac{b}{ab^2}$ to $\frac{b \cdot by}{ab^3}$. The attraction also of c on a is equal to that of b on a; therefore the whole attraction on a, in a direction towards o, will be expressed by $\frac{2b \cdot by}{ab^3}$. In the same manner we prove, that the attraction of a and c on b, in the direction bo, is $\frac{2a \cdot by}{ab^3}$; and that of a and b on c, in the direction co, is $\frac{2c \cdot by}{ab^3}$. Hence, a b and c being equal, the attractions in the directions ao bo and co will also be equal; and, consequently, in the direct ratio of these distances. Or rather, the hypothetical attractions being equal, it proves that, in order to revolve permanently, a b and c must be equal to each other.

Instead of moving in one circular orbit, the three stars may revolve in three equal ellipses, round their common centre of gravity, as in Fig. 6. And here we should remark, that this centre of gravity will be situated in the common focus o, of the three ellipses; and that the absolute attraction towards that focus, will vary in the inverse ratio of the squares of the distances of any one of the stars from that centre, while the relative attractions remain in the direct ratio of their several distances

from the same centre. This will be more fully explained, when we come to consider the motion of four stars.

A very singular straight-lined orbit, if so it may be called, may also exist in the following manner. If *a* and *b*, Fig. 7, are two large equal stars, which are connected together by their mutual gravitation towards each other, and have such projectile motions as would cause them to move in a circular orbit about their common centre of gravity, then may a third small star *c*, situated in a line drawn through *o*, and at rectangles to the plane described by the stars *a b*, fall freely from rest, with a gradually acquired motion to *o*; then, passing through the plane of the orbit of the two stars, it will proceed, but with a gradually retarded motion, to a second point of rest *d*; and, in this manner, the star *c* may continue to oscillate between *c* and *d*, in a straight line, passing from *c*, through the centre *o*, to *d*, and back again to *c*.

In order to see the possibility and permanency of this connection the better, let *o* be the centre of gravity of the three bodies, when the oscillating body is at *c*; then, supposing the bodies *a* and *b* to be at that moment in the plane *p l*, and admitting *m* to represent a body equal in mass to the two bodies *a b*, *o* will be the common centre of gravity of *m* and *c*. Then, by the force of attraction, the body *c* and the fictitious body *m* will meet in *o*; that is to say, the plane *p l*, of the bodies *a b*, will now be at *p' l'*. The fictitious body *m* may then be conceived to move on till it comes to *n*, while the body *c* goes to *d*; or, which is the same, the plane of the bodies *a b* will now be in the position *p'' l''*, as much beyond the centre of gravity *o*, as it was on the opposite side *m*. By this time, both the fictitious body *m*, now at *n*, and the real body *c*, now at *d*, have lost

their motion in opposite directions, and begin to approach to their common centre of gravity *o*, in which they will meet a second time. It is evident that the orbit of the two large stars will suffer considerable perturbations, not only in its plane, but also in its curvature, which will not remain strictly circular; the construction of the system, however, is such as to contain a sufficient compensation for every disturbing force, and will consequently be in its nature permanent.

In order to add an oscillating star, it is not necessary that the two large stars should be so situated as to move in a circular orbit, without the oscillating star. In Fig. 8, the stars *a* and *b* may have such projectile forces given them as would cause them to describe equal ellipses, of any degree of excentricity. If now the small star *c* be added, the perturbations will undoubtedly affect not only the plane of the orbits of the stars, but also their figures, which will become irregular moveable ovals. The extent also of the oscillations of the star *c* will be affected; and will sometimes exceed the limits *c d*, and sometimes fall short of them. All these varieties may easily be deduced from what has been already said, when Fig. 7 was considered. It is however very evident, that this system also must be permanent; since not only the centre of gravity *o* will always be at rest, but *a o*, whatever may be the perturbations arising from the situation of *c*, will still remain equal to *b o*.

It should be remarked, that the vibratory motion of the star *c* will differ much from a cometary orbit, even though the latter should be compressed into an evanescent ellipsis. For, while the former extends itself over the diameter of a globe in which it may be supposed to be inscribed, the hypothetical attractive force being supposed to be placed in its centre, the cometary

orbit will only describe a radius of the same globe, on account of its requiring a solid attractive centre.

After what has been said, it will hardly be necessary to add, that with the assistance of any proper one of the combinations pointed out in the four last figures, the appearance of every treble star may be completely explained; especially when the different inclinations of the orbits of the stars, to the line of sight, are taken into consideration.

If we admit of treble stars, we can have no reason to oppose more complicated connections; and, in order to form an idea how the laws of gravitation may easily support such systems, I have joined some additional delineations. A very short explanation of them will be sufficient.

Fig. 9 (Plate XVII.) represents four stars, *a b c* and *d*, arranged in a line; *a* being equal to *b*, and *c* equal to *d*. Then, if $ao = bo$, and $co = do$, the centre of gravity will be in *o*; and, with a proper adjustment of projectile forces, the four stars will revolve in two circular orbits round their common centre. By calculating in the manner already pointed out, it will be found, that when, for instance, $ao = 1$, $co = 3$, and $c = d = 1$, then the mass of matter in $a = b$, will be required to be equal to 1,3492.

It is not necessary that the projectile force of the four stars should be such as will occasion them to revolve in circles. The system will be equally permanent when they describe similar ellipses about the common centre of gravity, which will also be the common focus of the four ellipses. In Fig. 10, the stars *a b c d*, revolving in ellipses that are similar, will always describe, at the same time, equal angles in each ellipsis about the centre of hypothetical attraction; and, when they are removed from *a b c d* to *a' b' c' d'*, they will still be situated in a straight

line, and at the same proportionate distances from each other as before. By this it appears, as we have already observed, that the absolute hypothetical force in the situation $a' b' c' d'$, compared to what it was when the stars were at $a b c d$, is inversely as the squares of the distances; but that its comparative exertion on the stars, in their present situation, is still in a direct ratio of their distances from the centre o, just as it was when they were at $a b c d$; or, to express the same perhaps more clearly, the force exerted on a', is to that which was exerted on a as $\frac{1}{a'o|^2} : \frac{1}{ao|^2}$. But the force exerted on a is to that exerted on c, in our present instance, as $ao = 1$ to $co = 3$; and still remains in the same ratio when the stars are at a' and c'; for the exertion will here be likewise as $a'o = 1$ to $c'o = 3$.

Fig. 11 represents four stars in one circular orbit; and its calculation is so simple, that, after what has been said of Fig. 5, I need only remark that the stars may be of any size, provided their masses of matter are equal to each other.

It is also evident, that the projectile motion of four equal stars is not confined to that particular adjustment which will make them revolve in a circle. It will be sufficient, in order to produce a permanent system, if the stars $a b c d$, in Fig. 12, are impressed with such projectile forces as will make them describe equal ellipses round the common centre o. And, as the same method of calculation which has been explained with Figs. 6 and 10 may here be used, it will not be necessary to enter into particulars.

Fig. 13 represents four stars, placed so that, with properly adjusted projectile forces, they may revolve in equal times, and in two different circles, round their common centre of gravity o.

If $ao = bo = 4$, $co = do = 5$, and $c = d = 1$, then will the mass of matter in $a = b$, required for the purpose, be 1,5136. This arrangement, remarkable as it may appear, cannot be made in all situations; for instance, if the distance $ao = bo$ were assumed equal to 1, that of $co = do$ being 2, it would be impossible to find such quantities of matter in a and b as would unite the four stars into one system.

As we have shewn how the arrangement in Fig. 10 may be derived from that of Fig. 9, so it will equally appear, that four stars may revolve in different but similar ellipses round their common centre, as in Fig. 14. For here the four stars, when placed at $a\ b\ c\ d$, are exactly in the situation represented in Fig. 13; but, on account of different projectile forces, they revolve, not as before in concentric circles, but in similar elliptical orbits.

Fig. 15 represents three stars, $a\ b\ c$, in the situation of Fig. 5, to which a small oscillating star, d, is added. The addition of such a star to Fig. 1, has been sufficiently explained in Fig. 7; and, what has been remarked there, may easily be applied to our present figure. As the fictitious body m, in Fig. 7, was made to represent the stars a and b, it will now stand for the three stars $a\ b$ and c. If we suppose these stars to be of an equal magnitude in both figures, the centre of gravity o, of the three stars, will not be so far from m and n as in Fig. 7; and the perturbations will be proportionally lessened.

Fig. 16 gives the situation of three stars, $a\ b\ c$, moving in equal elliptical orbits about their common focus o, while the star d performs oscillations between d and e. What has been said in explaining Fig. 8, will be sufficient to shew, that the

present arrangement is equally to be admitted among the constructions of sidereal systems that may be permanent.

We have before remarked, that any appearance of treble stars might be explained, by admitting the combinations pointed out in Figs. 5, 6, 7, and 8; and it must be equally obvious, that quadruple systems, under what shape soever they may show themselves, whether in straight lines, squares, trapezia, or any other seemingly the most irregular configurations, will readily find a solution from one or other of the arrangements of the eight last figures.

More numerous combinations of stars may still take place, by admitting simple and regular perturbations; for then all sorts of erratic orbits of multiple flexures may have a permanent existence. But, as it would lead me too far, to apply calculation to them, I forbear entering upon the subject at present.

Before I proceed, it will be proper to remark, that it may possibly occur to many, who are not much acquainted with the arrangement of the numberless stars of the heavens, that what has been said may all be mere useless surmise; and that, possibly, there may not be the least occasion for any such speculations upon the subject. To this, however, it may be answered, that such combinations as I have mentioned, are not the inventions of fancy: they have an actual existence; and, were it necessary, I could point them out by thousands. There is not a single night when, in passing over the zones of the heavens by sweeping, I do not meet with numerous collections of double, treble, quadruple, quintuple, and multiple stars, apparently insulated from other groups, and probably joined in some small sidereal system of their own. I do not imagine that I have pointed out

the actual manner in which they are held together; but it will always be a desirable step towards information, if the possibility of such unions, in many different ways, can be laid before us; and, very probably, those who have more leisure to consider the different combinations of central forces, than a practical astronomer can have, may easily enlarge on what has been laid down in the foregoing paragraphs.

IV. *Of clustering Stars, and the Milky-way.*

From quadruple, quintuple, and multiple stars, we are naturally led to a consideration of the vast collections of small stars that are profusely scattered over the milky-way. On a very slight examination, it will appear that this immense starry aggregation is by no means uniform. The stars of which it is composed are very unequally scattered, and show evident marks of clustering together into many separate allotments. By referring to some one of these clustering collections in the heavens, what will be said of them will be much better understood, than if we were to treat of them merely in a general way. Let us take the space between β and γ Cygni for an example, in which the stars are clustering with a kind of division between them, so that we may suppose them to be clustering towards two different regions. By a computation, founded on observations which ascertain the number of stars in different fields of view, it appears that our space between β and γ, taking an average breadth of about five degrees of it, contains more than 331 thousand stars; and, admitting them to be clustering two different ways, we have 165 thousand for each clustering collection. Now, as a more particular account of the milky-way will be the subject of a separate paper, I shall only observe, that the above mentioned

milky appearances deserve the name of clustering collections, as they are certainly brighter about the middle, and fainter near their undefined borders. For, in my sweeps of the heavens, it has been fully ascertained, that the brightness of the milky-way arises only from stars; and that their compression increases in proportion to the brightness of the milky-way.

We may indeed partly ascribe the increase, both of brightness and of apparent compression, to a greater depth of the space which contains these stars; but this will equally tend to shew their clustering condition: for, since the increase of brightness is gradual, the space containing the clustering stars must tend to a spherical form, if the gradual increase of brightness is to be explained by the situation of the stars.

V. *Of Groups of Stars.*

From clustering stars there is but a short transition to groups of stars; they are, however, sufficiently distinct to deserve a separate notice. A group is a collection of closely, and almost equally compressed stars, of any figure or outline; it contains no particular condensation that might point out the seat of an hypothetical central force; and is sufficiently separated from neighbouring stars to shew that it makes a peculiar system of its own. It must be remembered, that its being a separate system does not exclude it from the action or influence of other systems. We are to understand this with the same reserve that has been pointed out, when we explained what we called insulated stars.

The construction of groups of stars is perhaps, of all the objects in the heavens, the most difficult to explain; much less can we now enter into a detail of the numerous observations I

have already made upon this subject. I therefore proceed in my enumeration.

VI. *Of Clusters of Stars.*

These are certainly the most magnificent objects that can be seen in the heavens. They are totally different from mere groups of stars, in their beautiful and artificial arrangement: their form is generally round; and the compression of the stars shews a gradual, and pretty sudden accumulation towards the centre, where, aided by the depth of the cluster, which we can have no doubt is of a globular form, the condensation is such, that the stars are sufficiently compressed to produce a mottled lustre, nearly amounting to the semblance of a nucleus. A centre of attraction is so strongly indicated, by all the circumstances of the appearance of the cluster, that we cannot doubt a single moment of its existence, either in a state of real solidity, or in that of an empty centre, possessed of an hypothetical force, arising from the joint exertion of the numerous stars that enter into the composition of the cluster.

The number of observations I have to give relating to this article, in which my telescopes, especially those of high space-penetrating power, have been of the greatest service, of course can find no room in this enumeration.

VII. *Of Nebulæ.*

These curious objects, which, on account of their great distance, can only be seen by instruments of great space-penetrating power, are perhaps all to be resolved into the three last mentioned species. Clustering collections of stars, for instance, may easily be supposed sufficiently removed to present

us with the appearance of a nebula of any shape, which, like the real object of which it is the miniature, will seem to be gradually brighter in the middle. Groups of stars also may, by distance, assume the semblance of nebulous patches; and real clusters of stars, for the same reason, when their composition is beyond the reach of our most powerful instruments to resolve them, will appear like round nebulæ that are gradually much brighter in the middle. On this occasion I must remark, that with instruments of high space-penetrating powers, such as my 40-feet telescope, nebulæ are the objects that may be perceived at the greatest distance. Clustering collections of stars, much less than those we have mentioned before, may easily contain 50000 of them; and, as that number has been chosen for an instance of calculating the distance at which one of the most remote objects might be still visible,* I shall take notice of an evident consequence attending the result of the computation; which is, that a telescope with a power of penetrating into space, like my 40-feet one, has also, as it may be called, a power of penetrating into time past. To explain this, we must consider that, from the known velocity of light, it may be proved, that when we look at Sirius, the rays which enter the eye cannot have been less than 6 years and $4\frac{1}{2}$ months coming from that star to the observer. Hence it follows, that when we see an object of the calculated distance at which one of these very remote nebulæ may still be perceived, the rays of light which convey its image to the eye, must have been more than nineteen hundred and ten thousand, that is, almost two millions of years on their way; and that, consequently, so many years ago,

* See Phil. Trans. for 1800, page 83. N. B. In the same page, line 22, for 5000 read 50000.

this object must already have had an existence in the sidereal heavens, in order to send out those rays by which we now perceive it.

VIII. *Of Stars with Burs, or Stellar Nebulæ.*

Situated as we are, at an immense distance from the remote parts of the heavens, it is not in the power of telescopes to resolve many phenomena we can but just perceive, which, could we have a nearer view of them, might probably shew themselves as objects that have long been known to us. A stellar nebula, perhaps, may be a real cluster of stars, the whole light of which is gathered so nearly into one point, as to leave but just enough of the light of the cluster visible to produce the appearance of burs. This, however, admits of a doubt.

IX. *Of milky Nebulosity.*

The phenomenon of milky nebulosity is certainly of a most interesting nature: it is probably of two different kinds; one of them being deceptive, namely, such as arises from widely extended regions of closely connected clustering stars, contiguous to each other, like the collections that construct our milky-way. The other, on the contrary, being real, and possibly at no very great distance from us. The changes I have observed in the great milky nebulosity of Orion, 23 years ago, and which have also been noticed by other astronomers, cannot permit us to look upon this phenomenon as arising from immensely distant regions of fixed stars. Even HUYGENS, the discoverer of it, was already of opinion that, in viewing it, we saw, as it were, through an opening into a region of light.* Much more would

* See *Systema Saturnium*, page 8 and 9.

he be convinced now, when changes in its shape and lustre have been seen, that its light is not, like that of the milky-way, composed of stars. To attempt even a guess at what this light may be, would be presumptuous. If it should be surmised, for instance, that this nebulosity is of the nature of the zodiacal light, we should then be obliged to admit the existence of an effect without its cause. An idea of its phosphorical condition, is not more philosophical, unless we could shew from what source of phosphorical matter, such immeasurable tracts of luminous phenomena could draw their existence, and permanency; for, though minute changes have been observed, yet a general resemblance, allowing for the difference of telescopes, is still to be perceived in the great nebulosity of Orion, even since the time of its first discovery.

X. *Of nebulous Stars.*

The nature of these remarkable objects is enveloped in much obscurity. It will probably require ages of observations, before we can be enabled to form a proper estimate of their condition. That stars should have visible atmospheres, of such an extent as those of which I have given the situation in this and my former catalogues, is truly surprising, unless we attribute to such atmospheres, the quality of self-luminous milky nebulosity. We can have no reason to doubt of the starry nature of the central point; for, in no respect whatever does its appearance differ from that of a star of an equal magnitude; but, when the great distance of such stars is taken into consideration, the real extent of the surrounding nebulosity is truly wonderful. A very curious one of this kind will be found in the 4th class, No. 69, of the annexed catalogue.

XI. *Planetary Nebulæ.*

This seems to be a species of bodies that demands a particular attention. To investigate the planetary nature of these nebulæ, is not an easy undertaking. If we admit them to contain a great mass of matter, such as that of which our sun is composed, and that they are, like the sun, surrounded by dense luminous clouds, it appears evidently that the intrinsic brightness of these clouds must be far inferior to those of the sun. A part of the sun's disk, equal to a circle of 15″ in diameter, would far exceed the greatest lustre of the full moon; whereas, the light of a planetary nebula, of an equal size, is hardly equal to that of a star of the 8th or 9th magnitude. If, on the other hand, we should suppose them to be groups, or clusters of stars, at a distance sufficiently great to reduce them to so small an apparent diameter, we shall be at a loss to account for their uniform light, if clusters; or for their circular forms, if mere groups of stars.

Perhaps they may be rather allied to nebulous stars. For, should the planetary nebulæ with lucid centres, of which the next article will give an account, be an intermediate step between planetary nebulæ and nebulous stars, the appearances of these different species, when all the individuals of them are fully examined, might throw a considerable light upon the subject.

XII. *Of planetary Nebulæ with Centres.*

In my second catalogue of nebulæ, a single instance of a planetary nebula with a bright central point was mentioned; and, in the annexed one, No. 73 of the 4th class, is another of very nearly the same diameter, which has also a lucid, though

not quite so regular a centre. From several particularities observed in their construction, it would seem as if they were related to nebulous stars. If we might suppose that a gradual condensation of the nebulosity about a nebulous star could take place, this would be one of them, in a very advanced state of compression. A further discussion of this point, however, must be reserved to a future opportunity.

CORRECTION OF A FORMER PAPER.

In my Paper on two lately discovered celestial Bodies,
Page 224, line 18, of this volume, instead of 135, read 31.

Catalogue of 500 additional new Nebulæ, and Clusters of Stars.

First Class. Bright Nebulæ.

I.	1788.	Stars.		M.	S.		D.	M.	♀	Description.
216	Dec. 3	22 Ursæ	*p*	13	58	*f*	3	42		*v*B. *p*L. *i*F. *r. mb*M. Towards the *ff*, within the nebulosity, is a *v*S. *ft*.
217		2754 Persei	*f*	9	25	*n*	0	46	2	*c*B. *c*L. *mb*M. Stands nearly in the center of a trapezium.
218		3168 Aurigæ	*f*	26	48	*f*	0	20	1	*c*B. R. *vgmb*M. about 3′ *d*.
	1789									
219	Mar. 28	55 Ursæ	*f*	5	33	*n*	0	36	1	*v*B. *c*L. *i*F. *vgmb*M.
220	Apr. 12	64 (γ) Ursæ	*p*	48	59	*f*	0	20	2	*c*B. *m*E. 70° *np ff*. 3 or 4′ *l*, 2′ *b*.
221	—	— —	*p*	21	41	*f*	0	37	2	*c*B. R. *vgmb*M, 4 or 5′ *d*.
222	—	— —	*p*	20	20	*f*	0	35	2	*c*B. *i*E. near mer. *gb*M. 2′ *l*.
223	—	— —	*f*	6	4	*f*	2	45	2	*v*B. *m*E. *np ff*. BN. 5′ *l*. 1½′ *b*.
224	—	1 Canum	*p*	9	19	*f*	8	10	2	*c*B. *p*L. *m*E. SN.
225	—	— —	*p*	8	31	*f*	0	46	2	*v*B. *p*L. BrN. just *f a cft*.
226		1464 (γ) Ursæ	*p*	38	32	*f*	0	34	1	*c*B. R. SBrN and *v*F chev. 4′ *d*.
227	—	— —	*p*	15	28	*n*	2	37	2	*c*B. *c*L. *i*F. *r. vgb*M. 3′ *l*. 2′ *b*.
228	—	— —	*p*	5	20	*n*	2	24	2	*v*B. *v*B*i*N. and F. bran. 1½′ *l*. ¾′ *b*.
229	—	— —	*f*	3	46	*n*	1	47	1	The 2d of 2. *v*B. R. *vgb*M. See II. 791.
230	—	83 Ursæ	*f*	20	24	*n*	0	27	2	*c*B. S. E. *fp nf*. *c*BN. and F bran.
231	—	— —	*f*	24	34	*n*	0	10	2	*c*B. *p*S. *i*R.
232	—	— —	*f*	27	7	*n*	0	16	1	The 2d of 2. *c*B.S.R. *vgmb*M. See III. 791.
233		1744 Ursæ	*f*	1	14	*f*	0	16	2	*c*B. E. 30° *fp nf*. *r. mb*M. 3′ *l*. 1½′ *b*.

I.	1789.	Stars.		M.	S.		M.	D	♀	Description.
234	Apr. 17	74 Ursæ -	*f*	1	31	*f*	0	28	2	*c*B. S. *l*E. Just *p a p*L *ſt.*
235	—	12 (*ι*) Draconis	*p*	66	52	*f*	2	32	2	*c*B. *i*F *vgmb*M. 7′ *l*, 5′ *b.*
236	—	— —	*p*	59	56	*f*	2	133	3	*v*B. S. *i*R. *Bir*N. *vgmb*M.
237	—	— —	*p*	54	10	*f*	0	52	1	B. *i* oval. *vgmb*M.
238		24 69 Ursæ Hev.	*f*	27	55	*f*	0	32	2	*c*B. *p*L. *i*R *vgmb*M.
239	—	— —	*f*	28	10	*f*	0	173	2	*c*B. *p*L. E. *mb*M.
240	—	— —	*f*	28	84	*f*	0	172	2	*c*B. *p*L. E. SBN.
	1790									
241	Feb. 17	19 (*ξ*) Hyd. Crat.	*p*	14	48	*f*	0	57	1	*c*B. E. 70° *np ff*. *vgb*M 7′ *l*, 4′ *b.* within a parallelogram.
242	Mar. 17	15 (*f*) Ursæ	*p*	15	40	*f*	0	211	1	*v*B. LB*r*N. with *v*F chev.
243	—	77 (*ε*) Ursæ	*f*	1	47	*n*	2	251	2	*c*B. S. R. *gb*M.
244	18	39 Ursæ -	*f*	36	44	*n*	0	402	2	*c*B. R. *vgmb*M. 1½′ *d.*
245	—	— —	*f*	39	27	*n*	1	588	3	*v*B. *c*L. R. *vgb*M.
246	—	66 Ursæ -	*p*	29	19	*n*	0	202	2	*c*B. *p*L. E.
247	—	— —	*p*	28	18	*n*	2	02	2	*v*B. *p*L. *l*E. near par. *mb*M.
248	—	— —	*p*	7	5	*n*	2	522	2	*c*B. *p*L. *i*F.
249	19	17 Ursæ -	*p*	9	0	*n*	3	482	2	*c*B. E. near par. *er.* *b*M. 4′ *l*, 2′ *b.* I suppose, with a higher power and longer attention, the stars would become visible.
250	—	— —	*p*	4	47	*n*	3	171	1	*v*B. *c*L. *l*E. LBNM.
251	—	76 Ursæ -	*p*	50	48	*f*	2	31	1	*v*B. perfectly R. BN and F chev. *vgb*M. 1½′ *d.*
252	—	— —	*p*	41	11	*f*	0	341	1	*v*B. *c*L. R.
253	—	— —	*p*	41	46	*f*	0	511	1	*v*B. *v*L. E.
254	—	— —	*p*	1	47	*f*	1	81	1	*c*B. E. par. 5′ *l.* all over equally B. except just on the edges.
255	—	69 Ursæ Hev.	*f*	19	26	*n*	1	11	1	*v*B. BENM. 3′ *l.* ½′ *b.*
256	—	— —	*f*	21	38	*n*	0	181	1	*v*B. *p*L. *i*F. suddenly *mb*M.
257	Oct. 9	12 Eridani	*f*	16	58	*f*	1	581	1	*c*B. *i*R *vgmb*M. 1½′ *d.*
258	Dec. 28	47 (*λ*) Persei	*p*	3	41	*n*	1	0	1	*v*B. *i*F. *r. b*M. 5′ *l.* 4′ *b.* A *p*L star in it towards the *f* side, but unconnected.

I.	1791.	Stars.		M.	S.		D.	M.		Description.
259	Mar. 7	17 Hydræ Crat.	f	18	31	n	0	27	1	cB. pL. lE. gbM. The brightness takes up a large space of it.
260	Apr. 2	28 (b) Ursæ	p	1	49	f	0	34	1	vB. vS. iR. mbM.
	1793									
261	Feb. 4	38 of the *Connois.*	f	8	7	f	1	35	1	vB. iR. vgbM. 5′d. Seems to have 1 or 2 stars in the middle, or an *i* N; the chev. diminishes vg.
262	Apr. 6	1 (λ) Draconis	p	2	6	f	2	41	1	cB. vS. iF. N. with vF. chev.
263	—	4 Draconis	p	22	48	f	0	23	1	cB. lE. bM.
264	7	— —	p	14	18	n	1	36	1	cB. S. bM.
265	8	37 Ursæ	p	16	16	n	1	51	1	cB. S. iR. vgmbM.
266	—	— —	p	13	35	f	0	11	1	cB. pL. iF. gbM.
267	—	39 Ursæ	f	11	21	f	0	10	1	cB. pL. iR. 1½′d. The greatest part of it almost equally B.
268	—	— —	f	12	46	f	0	41	1	vB. vS. R. Stellar.
269	—	— —	f	18	1	n	0	29	1	cB. R. 1′d. just *n* of a S*f*t.
270	—	— —	f	35	36	n	1	42	2	vB. cL. E. par. SN. E par.
271	—	— —	f	35	54	n	0	55	1	vB. cL. E. mbM.
	1796									
272	Mar. 4	Georgian planet	p	0	53	n	0	6	2	cB. S. iR. BN. mbM. This nebula was seen at 9ʰ 27′, sidereal time; the telescope being out of the meridian.
273	Nov. 22	A double star	f	5	45	f	0	39	3	vB. vL. E. near par. The determining star follows 5 Draconis Hevelii 13′ 54″ in time, and is 0° 23′ more south.
274	—	— —	f	10	13	f	0	24	3	cB. vS. iR. bM.
275	Dec. 10	5 Dracon. Hev.	f	1	32	n	0	12	2	cB. S. R.
276	—	— —	f	2	45	n	0	12	2	cB. cL. iF. lE. mbM.
277	—	— —	f	6	20	n	0	20	2	vB. cL. lE. mbM.
278	12	— —	p	11	5	f	0	15	1	cB. cL. iR. mbM.

I.	1796.	Stars.		M.	S.		D.	M		Description.
279	Dec. 12	— —	*p*	10	28	*n*	1	38	2	*c*B. *c*L. *l*E. *b*M.
280	—	16 (ζ) Ursæ min.	*f*	51	33	*n*	0	33	1	*v*B. *c*L. *l*E. *b*M. The greatest brightnes confined to a small point.
	1798									
281	Dec. 9	Apps. Sculps. L. C. 95 -	*p*	1	47	*n*	0	27	1	*c*B. E. *np ff.* NM. 6′*l.* 1½′*b.*
	1801									
282	Apr. 2	208 (N) Camelop. of Bode's Cat.	*p*	153	15	*f*	2	48	1	*c*B. *p*L. *i*F.
283	—	— —	*p*	113	40	*f*	3	41	1	*c*B. *c*L. *er.*
284	—	— —	*p*	85	18	*f*	0	23	1	*c*B. *v*S. *i*F.
285	Nov. 8	24 (*d*) Ursæ	*f*	13	14	*f*	2	53	1	*v*B. *v*L. E. *np ff.* 6′*l.* 2′*b.*
286	—	— —	*f*	30	0	*f*	1	81	1	*v*B. *c*L. R. *vgmb*M. On the north following side there is a F ray interrupting the roundness.
287	Dec. 7	1 (λ) Draconis	*p*	4	37	*n*	1	13	1	*c*B. *m*E. *np ff. mb*M. 3′*l*, 1′*b.*
	1802									
288	Sept. 26	184 Camelopar. of Bode's Cat.	*p*	11	38	*f*	2	34	1	*v*B. *c*L. *l*E. suddenly *mb*M.

Second Class. Faint Nebulæ.

II.	1789.	Stars.		M.	S.		D.	M		Description.
769	Feb. 22	81 (*g*) Geminor.	*p*	37	58	*n*	0	41	1	*p*B. *p*L. *i*R. *er. b*M.
770	—	62 Ursæ -	*p*	13	44	*f*	2	15	1	*p*B. *p*L. R. *lb*M.
771	Mar. 20	26 (χ) Virginis	*p*	7	0	*n*	0	26	2	*p*B. *c*L. *i*F. *er. mb*M. ¼ or 5′*d.*
772	—	— —	*f*	3	9	*n*	0	57	2	F. S. E.
773	—	— —	*f*	3	5	*n*	1	1	2	F. S. E. *b*M.
774	—	— —	*f*	6	27	*n*	0	55	2	*p*B. S. *i*R. *mb*M.
775		2855 Ursæ -	*f*	3	31	*f*	0	25	1	*p*B. *c*L. *l*E. *vgmb*M.
776	—	26 (χ) Virginis	*p*	8	19	*f*	0	41	1	F. *v*L. *er.*
777	—	— —	*f*	17	15	*n*	1	91	1	F. S. R. *b*M.
778	—	— —	*f*	21	12	*n*	1	54	1	F. S. *ff.* a double star.
779	—	26 (χ) Virginis	*f*	22	44	*n*	0	14	1	F. S.
780		2646 (γ) Hydræ	*f*	1	22	*f*	1	14	1	F. R. *r. vglb*M. ¼*d.*

II.	1789.	Stars.		M.	S.		D.	M.	P	Description.
781	Apr. 12	1 Canum -	*p*	10	55	*s*	0	53	2	A *p*S. *st.* involved in nebulosity of no great extent; the *st.* does not seem to belong to it.
782	14	64 (γ) Ursæ	*p*	31	7	*n*	0	7	1	*p*B. S. R. *vgmb*M. just *f* a S*t.*
783	—	— —	*p*	18	40	*n*	0	50	1	*p*B. *p*L. *b*M.
784	—	— —	*p*	17	41	*n*	0	37	1	*p*B. *c*L. *l*E. 3′*l.*
785	—	— —	*p*	7	8	*n*	1	18	1	*p*B. S. *l*E.
786	—	— —	*p*	8	31	*n*	1	39		F. E.
787	—	— —	*p*	8	8	*n*	1	27	1	Two nebulæ; the 1st *p*B. S.
788				8	7		1	24		The 2d *p*B. S.
789	—	— —	*f*	1	35	*n*	1	38	1	Two nebulæ; the 1st *p*B. E.
790										The 2d F. S.
791	—	— —	*f*	3	24	*n*	1	48	1	The 1st of 2. *p*B. S. E. See I. 229.
792	—	1 Canum -	*p*	8	19	*n*	2	47	1	F. S. R. *b*M.
793	—	— —	*p*	0	57	*n*	2	36	2	F. *p*L. *i*F. *b*M.
794	—	77 (*s*) Ursæ	*p*	11	32	*f*	0	49	2	F. S.
795	—	— —	*p*	8	25	*f*	1	15	2	*p*B. *v*S. *mb*M.
796	—	— —	*p*	7	20	*f*	1	25	2	*p*B. *c*S. *l*E. B*r*N.
797	—	81 Ursæ -	*p*	3	33	*f*	2	18	2	*p*F. *p*S. R. *vgb*M.
798	—	83 Ursæ -	*f*	0	49	*n*	1	1	1	*p*B. E. $1\frac{1}{2}l$, $\frac{1}{2}b$.
799	—	— —	*f*	21	27	*n*	1	7	2	*p*B. *c*L. E.
800	—	— —	*f*	25	7	*n*	1	2	1	*p*B. S.
801	—	— —	*f*	27	27	*n*	0	23	2	F. *c*L.
802	17	71 Ursæ -	*p*	15	20	*n*	1	33	1	F. S. E.
803	—	— —	*p*	13	57	*n*	0	59	2	F. S. R.
804	—	— —	*p*	5	43	*s*	0	3	1	*p*B. *p*L. *i*F.
805	—	— —	*p*	4	41	*n*	1	20	1	The 2d of 2. *p*B. *p*L. *mb*M. See III. 798.
806	—	— —	*p*	2	13	*n*	1	42	1	*p*B.
807	—	12 (*i*) Draconis	*p*	55	48	*n*	0	42	1	*p*B. E. *mer.* $1\frac{1}{2}l$, $\frac{1}{2}b$.
808	24	Neb. II. 756	*p*	24	16	*n*	0	41	1	*p*B. S. *i*F. *er.* mixed with some *p*L. stars, which may perhaps belong to it.
809	—	— —	*p*	15	5	*s*	0	26	1	F. S. E.

II.	1789.	Stars.		M.	S.		D.	M	Q	Description.
810	Apr. 24	21 (μ) Draconis	*p*	46	31	*n*	3	23	1	*p*F. *p*S. *l*E.
811	—	— —	*p*	44	9	*n*	0	50	1	*p*B. *i*R. *vgvlb*M.
812	—	— —	*f*	10	4	*n*	2	55	1	F. S. R. *vglb*M.
813	26	5 Canum	*p*	10	58	*f*	0	50	1	*p*B. S. *l*E.
814	—	7 Canum	*f*	20	24	*n*	0	20	1	F. S. *vfmb*M.
815	—	82 Ursæ -	*p*	31	48	*f*	0	52	1	F. *v*S. Stellar.
816	—	— —	*p*	26	52	*f*	1	36	1	F. S. *i*R. *vgmb*M.
817	—	— —	*p*	8	42	*f*	1	40	1	*p*B. S. R. *vgb*M.
818	—	12 Draconis	*p*	40	16	*n*	0	38	1	*p*F. *c*S. R. *vgb*M.
	1790									
819	Mar. 8	13(λ)Hyd.Crat.	*p*	11	58	*n*	0	31	1	*p*F. *p*L. *i*F. *b*M.
820	10	65 Aurigæ	*f*	7	22	*n*	0	11		*p*B. S. Stellar.
821	—	70 Geminorum	*p*	1	43	*n*	0	12	1	*p*B. *c*S. *r. p a cst.*
822	17	27 Lyncis -	*p*	25	42	*n*	0	41	1	*p*F. R. *r. vgb*M.
823	—	15 (*f*) Ursæ	*p*	12	10	*f*	0	18	1	*p*B. S. R. *mb*M.
824	—	26 Ursæ -	*f*	139	17	*f*	0	11		*p*B. *m*E. *6'l*, *2'b.*
825	—	— —	*f*	139	40	*f*	1	44	1	*p*B. S. *i*F. *b*M.
826	—	77 (ι) Ursæ	*f*	28	0	*n*	1	42	1	F. S. E.
827	—	— —	*f*	69	19	*n*	3	27	1	*p*B. S. *i*F. *mb*M.
828	18	17 Ursæ -	*p*	6	25	*f*	2	57	1	*p*B. S. *vgmb*M.
829	—	66 Ursæ -	*p*	31	14	*n*	1	9	2	F. E. *np ff. er.* $1\frac{1}{2}'l.$
830	—	— —	*p*	15	23	*f*	0	20	1	*p*B. E.
831	—	— —	*p*	11	44	*n*	1	22	1	*p*B. *v*S. *l*E.
832	—	— —	*p*	6	58	*n*	2	52	2	*p*B. *p*L. R. The nebulosity of this runs into that of I. 248.
833	—	— —	*p*	1	1	*n*	1	46	1	F. S.
834	19	17 Ursæ -	*p*	11	34	*n*	3	10	1	*p*F. *p*S. *i*F. *er.*
835	—	29 (υ) Ursæ	*f*	5	11	*n*	0	15	2	F. S. E. near par.
836	—	76 Ursæ -	*p*	70	41	*f*	0	58	1	F. S. R. *r.* almost of equal light throughout.
837	—	— —	*p*	66	54	*f*	1	0	1	*p*B. *l*E.
838	—	— —	*p*	66	15	*f*	3	9	1	*p*B. S.
839	—	— —	*p*	63	0	*f*	2	28	1	*p*B. *c*S. R. *mb*M.
840	—	— —	*p*	47	30	*f*	2	16	1	F. S. *b*M.
841	—	69 Ursæ Hev.	*f*	4	24	*n*	2	46	2	The 1st of 2. *p*B. S. *i*F.
842	—	— —	*f*	4	35	*n*	2	50	2	The 2d of 2. *p*B. *p*L. *i*F.

H.	1790.	Stars.		M.	S.		D.	M.		Description.
843	Mar. 19	— —	*f*	26	40	*n*	0	42	1	F. S.
844	—	— —	*f*	27	43	*f*	0	29	1	*p*B. *c*L.
845	20	50 (*ω*) Ursæ	*f*	22	41	*n*	1	44	8	*p*B. *p*L. *i*R. *b*M.
846	—	76 Ursæ -	*p*	23	9	*n*	3	13	1	*p*B. *m*E. *sp nf*. BN. $5'l, \frac{1}{2}'b$.
847	—	— —	*p*	19	1	*n*	3	8	1	*p*B. S. *l*E.
848	—	— —	*p*	14	21	*n*	2	8	1	F. *i*F. *b*M. Stellar.
849	—	— —	*p*	9	7	*n*	1	15	1	*p*B. *v*S. *l*E. SN.
850	—	— —	*p*	7	16	*n*	0	48	1	*p*B. *p*L. *i*R. *r*. *vgb*M.
851	Oct. 9	72 Pegasi -	*f*	18	8	*f*	0	6	2	*p*F. *p*L. *i*R. *lb*M. *sp*. a *v*S*st*.
852	—	*σ* Fornacis L. C. 285 - -	*p*	4	15	*f*	0	34	1	F. *p*L. *i*R. *gb*M.
853	Nov. 26	29 (*π*) Androm.	*p*	25	38	*f*	0	24	1	F. S. E. near mer.
854	Dec. 25	44 Piscium -	*f*	3	49	*n*	0	56	1	*p*B. *v*S. R. *vgmb*M. pretty well defined on the margin.
855	—	— —	*f*	4	44	*n*	0	10	2	*p*B. *c*L. *i*R. *r*. *vgb*M. *sp*. a *v*S*st*.
856	—	— —	*f*	13	52	*n*	1	8	1	F. S. *vgb*M.
857	—	— —	*f*	13	52	*n*	0	53	1	F. S. *vgb*M.
858	—	— —	*f*	14	10	*n*	0	58	1	*p*B. S. *vgb*M.
859	—	98 (*μ*) Piscium	*f*	20	28	*n*	0	1	1	*p*B. S. E. near par. *sp*. a S*st*.
860	28	MAYER's Zod. Cat. No. 18	*p*	5	48	*n*	0	39	1	*p*F. *v*S. *vgb*M.
861	—	57 Aurigæ -	*f*	17	30	*n*	1	54	1	*p*B. *p*L. *i*F. *gb*M.
862	—	— —	*f*	23	5	*n*	1	29	1	F. *p*L.
863	29	63 (*δ*) Piscium	*p*	0	39	*n*	0	44	1	- - - *p*L. *l*E. *r*. *gb*M.
864	1791 Mar. 7	17 Hyd. Crat.	*f*	16	46	*f*	0	1	1	*p*B. S. R. *vgmb*M. almost resembling a N.
865 } 866	—	— —	*f*	34	2	*f*	0	31	1	Two nebulæ, both F. S. R. *b*M. and nearly in the same par.
867	April 2	73 Ursæ -	*p*	14	8	*f*	1	12	1	*p*B. *v*S. Stellar.
868 } 869	3	14 (*τ*) Ursæ	*f*	10 38 11 8		*n*	0	47	1	Two nebulæ, the 1st F. S. *i*F. The 2d F. *p*L. E.

II.	1791.	Stars.		M.	S.		D.	M.	♀	Description.
870	April 8	35 Ursæ -	*f*	2	50	*f*	0	36	1	F. S. *i*R. Almost of equal light throughout.
871	—	— —	*f*	3	37	*f*	0	52	1	F. *v*S. *mb*M.
872	—	— —	*f*	21	30	*n*	0	11	1	F. *c*L. *i*R.
873	May 6	13 (γ) Ursæ min.	*f*	37	53	*f*	1	17	1	F. R. *b*M. 1′*d*.
874	24	37 (ξ) Bootis	*f*	34	48	*f*	1	12	1	*p*B. *p*L. *i*R. *vgmb*M.
875	30	25 Herculis	*f*	3	10	*n*	2	12	1	*p*B. S. *l*E. *vgmb*M.
	1792									
876	Apr. 20	22 (*f*) Bootis	*p*	15	58	*n*	0	26	1	*p*B. *v*S.
877	—	— —	*p*	18	27	*n*	1	21	1	*p*B. *p*L. *i*F.
878	Sept. 16	3 Cephei Hev.	*p*	29	15	*f*	0	25	1	*p*B. *i*F. *b*M. contains 2 stars.
	1793									
879	April 6	1 (λ) Draconis	*p*	9	49	*f*	2	5	1	*p*B. S. R. *b*M.
880	—	— —	*p*	7	44	*n*	0	6	2	F. S. *l*E. *sp nf*. but near mer. *gb*M.
881	7	4 Draconis	*p*	45	43	*n*	0	12	1	F. *m*E. *np ff*. but near par. $1\frac{1}{2}'l$.
882	8	37 Ursæ -	*p*	10	40	*n*	1	8	1	*p*B. *p*L. *l*E. *b*M.
883	—	— —	*p*	8	36	*n*	0	8	1	F. S. R. *b*M.
884	—	39 Ursæ -	*f*	22	42	*f*	0	37	1	F. S. R. *b*M.
885	—	— —	*f*	37	41	*n*	0	42	1	F. S. *l*E. *np ff*.
886	—	— —	*f*	44	5	*f*	0	21	1	*p*B. *i*F.
887	9	42 Ursæ -	*f*	2	41	*n*	1	56	1	F. *p*L. *i*F. *b*M.
888	—	— —	*f*	7	21	*n*	0	11	1	F. S. R. *b*M.
889	May 12	19 Bootis Hev.	*p*	26	45	*n*	0	20	1	*p*B. *p*L. R. just foll. a S*st*.
890	—	— —	*p*	13	20	*n*	0	33	1	*p*B. *p*L. *i*R.
891	—	— —	*f*	6	44	*n*	0	8	1	*p*B. *p*L. *l*E. BM.
892	—	— —	*f*	7	44	*n*	0	24	1	F. S. E. near mer.
893	—	— —	*f*	9	37	*f*	0	22	1	*p*B. S. *i*F.
894	—	— —	*f*	10	46	*f*	0	31	1	F. S.
895	13	93 (τ) Virginis	*p*	21	54	*f*	0	40	1	F. S. *i*R.
896	—	— —	*p*	21	49	*f*	0	40	1	F. S. *i*R.
897	Sept. 6	58 Aquarii	*p*	16	29	*n*	0	7	1	*p*B. *l*E. *r*. $1\frac{1}{4}'l$. $1\frac{1}{4}'b$.
	1794									
898	Mar. 22	Georgian planet	*f*	3	0	*n*	0	33	1	F. 8′ north of a *p*L. red *ft*. This nebula was seen at 8^h 49′, sidereal time, the

II.	1797.	Stars.		M.	S.		D.	M.	♀	Description.
										telescope being out of the meridian.
899	Dec. 20 1798	4 (*b*) Ursæ min.	*p*	26	13	*f*	0	40	1	F. S. E. near mer. 1′*l*.
900	Dec. 10 1799	18 (*s*) Eridani	*p*	20	58	*f*	1	5	1	F. E. *fp nf*. near par. 3′*l*, 1′*b*.
901	June 29	93 Herculis -	*p*	27	30	*f*	0	11	1	F. S. *i*F. *er*. 2′*l*.
902	—	— —	*f*	7	47	*n*	0	49	1	F. *p*L. R. *vgb*M. 3½′*d*.
903	1801 April 2	208(N) Camelop. of Bode's Cat.	*p*	139	19	*f*	1	39	1	F. *p*L. *r*.
904	—	— —	*p*	68	9	*f*	1	58	1	F. *p*L. *lb*M.
905	—	— —	*p*	36	53	*f*	2	22	1	*p*B. *p*L.
906	Nov. 28 1802	11 (*α*) Draconis	*f*	86	13	*n*	0	8	1	F. S. *l*E. *fp nf*. *vglb*M.
907	June 26	2 (*μ*) Lyræ	*f*	5	21	*n*	0	18	1	F. S. *i*F.

Third Class. Very faint Nebulæ.

III.	1788.	Stars.		M.	S.		D.	M.	♀	Description.
748	Dec. 3	43 Camelop.	*f*	35	5	*n*	0	29	1	*v*F. *v*S. has a *v*F. bran *nf*.
749	—	22 Ursæ -	*p*	12	45	*f*	0	24	1	*c*F. *v*S.
750	31	63 Aurigæ -	*f*	48	58	*n*	0	48	1	*v*F. S. R. *lb*M.
751	—	39 Lyncis -	*f*	25	15	*f*	0	30	2	*e*F. S.
752	1789 Feb. 22	16 (*ζ*) Cancri	*p*	4	19	*n*	0	8	1	*e*F. *l*E. *f* of a *v*S*st*.
753	—	33 (*η*) Cancri	*p*	8	11	*f*	0	4	1	*v*F. S. R. *vlb*M.
754	24	6 Corvi -	*p*	17	38	*f*	1	43	1	*e*F. *v*S. R.
755	Mar. 20	96 (*χ*) Virginis	*p*	13	3	*n*	0	20	1	Two nebulæ, both *v*F. *v*S. E. within 1½′ of each other.
756										
757	—	— —	*p*	5	25	*n*	0	38	2	2 *v*S. stars involved in *v*F. nebulosity of no great extent.

III.	1789.	Stars.		M.	S.		D.	M.		Description.
758	Mar.	— —	ſ	20	55	*n*	1	53	1	Two nebulæ, both *v*F. *v*S.
759	28									
760	—	— —	ſ	23	47	ſ	0	9	1	*c*F. *v*S. R.
761	—	— —	ſ	24	55	*n*	0	18	1	*v*F. S.
762	—	102 (ν') Virginis	*p*	11	30	*n*	0	36	1	*v*F. *v*S.
763	—	105 (φ) Virginis	*p*	1	1	ſ	0	1	1	*e*F. S.
764	26	9 (β) Corvi	*p*	4	55	*n*	0	15	1	*c*F. *p*S. R. Stellar.
765	—	45 (ψ) Hydræ	*p*	1	35	ſ	0	53	1	*v*F. *p*L. *i*F.
766	—	— —	ſ	0	39	ſ	0	16	1	*v*F. *v*S.
767	Apr. 12	64 (γ) Ursæ	*p*	78	24	ſ	3	45	1	*v*F. *p*S. *i*E.
768	—	— —	*p*	30	48	ſ	0	49	2	*v*F. *v*S. Stellar.
769	—	— —	*p*	1	40	ſ	1	44	1	*c*F. S.
770	14	— —	*p*	39	32	*n*	2	2	1	*v*F. *v*S. Stellar.
771	—	— —	*p*	19	37	*n*	1	8	1	*e*F. S. *i*E. On account of the brightness of 179 Ursæ maj. of BODE's Cat. which was in the field of view with it, I had nearly overlooked it.
772	—	— —	*p*	19	2	*n*	1	16	1	*v*F. Stellar.
773	—	— —	*p*	14	0	*n*	2	32	1	*c*F *p*S. *l*E. just ſ a *v*S*ſt*.
774	—	— —	*p*	10	37	ſ	0	58	2	*v*F. S.
775	—	— —	*p*	10	17	ſ	1	1	1	*v*F. *v*S.
776	—	— —	*p*	9	33	*n*	2	12	1	*e*F. *p*L. *l*E.
777	—	1 Canum -	*p*	1	54	ſ	0	33	1	*c*F. S. Stellar.
778	—	77 (ι) Ursæ	*p*	9	10	ſ	1	4	2	*c*F. S. *l*E. *i*F.
779	—	— —	ſ	11	36	*n*	0	20	2	*v*F. S.
780	—	— —	ſ	12	37	ſ	0	19	1	*c*F. S.
781	—	— —	ſ	12	30	ſ	2	22	1	Two nebulæ. Both *v*F. S.
782	—			12	44		2	20		
783	—	— —	ſ	12	33	ſ	2	28	1	*v*F. S. E.
784	—	81 Ursæ -	*p*	7	6	*n*	0	9	1	*c*F. S. *i*R.
785	—	83 Ursæ -	ſ	4	34	*n*	0	37	1	2 *e*F. *ſt*. with nebulosity.
786	—	— —	ſ	14	8	ſ	0	22	1	*v*F. *v*S. Stellar.
787	—	— —	ſ	22	27	ſ	0	28	1	*v*F. *v*S.
788	—	— —	ſ	23	47	ſ	0	24	1	*v*F. *v*S.
789	—	— —	ſ	23	54	ſ	0	22	1	*v*F. *v*S.

III.	1789.	Stars.		M.	S.		D.	M.	♀	Description.
790	Apr. 14	83 Ursæ	*f*	25	23	*f*	0	17	1	*v*F. *p*L.
791	—		*f*	27	7	*n*	0	16	1	The 1st of 2. *v*F. S. 4′ dist. from I. 232.
792	17	44 Ursæ	*p*	2	11	*n*	0	50	1	*v*F. S. E. 20° *sp nf. er.*
793	—	48 (β) Ursæ	*f*	1	25	*f*	0	10	1	*v*F. *v*S. Stellar. The brightness of β Ursæ is so considerable, that it requires much attention to perceive this nebula.
794	—	71 Ursæ	*p*	22	30	*n*	1	8	1	*c*F. S. ver 300.
795	—	— —	*p*	16	8	*n*	2	52	1	*v*F. S. *i*F. *r.*
796	—	— —	*p*	11	28	*n*	2	52	1	*e*F.
797	—	— —	*p*	10	56	*n*	3	11	2	*v*F. S.
798	—	— —	*p*	5	4	*n*	1	20	1	The 1st of 2. *c*F. *l*E. *i*F. II. 805.
799	—	— —	*p*	1	12	*n*	1	36	1	*v*F. *v*S.
800	} —	— —	*p*	1	9	*n*	1	37	1	{ Two nebulæ, both *e*F. *c*S. R.
801	}									
802	—	74 Ursæ	*f*	4	54	*n*	0	30	2	The 1st of 2. *v*F. S. *l*E. See III. 807.
803	—	69 Ursæ Hev.	*f*	9	33	*f*	2	53	2	*e*F. *v*S.
804	—	— —	*f*	46	59	*f*	2	18	2	*e*F. S. E. *r.*
805	—	— —	*f*	48	9	*f*	0	13	2	*e*F. *v*S. R. Stellar.
806	—	12 (ι) Draconis	*p*	34	20	*n*	0	8	1	*v*F. *v*S. *l*E.
807	24	74 Ursæ	*f*	5	26	*n*	0	34	1	The 2d of 2. *e*F. S. E. differently from III. 802.
808	—	69 Ursæ Hev.	*p*	7	35	*f*	2	19	1	*c*F. S. E.
809	—	— —	*f*	27	7	*f*	1	25	1	*v*F. *v*S.
810	—	— —	*f*	30	44	*f*	0	13	1	*c*F. *v*S. R.
811	—	Neb. II. 756	*f*	0	32	*n*	0	2	1	*v*F. S. E.
812	—	21 (μ) Draconis	*p*	55	20	*n*	3	18	1	*v*F. *v*S. *l*E.
813	—	— —	*p*	36	1	*n*	1	14	1	*v*F. *v*S. *i*R.
814	26	5 Canum	*p*	15	0	*n*	0	32	1	*v*F. S. *er.*
815	—	7 Canum	*f*	18	48	*f*	0	22	1	S. Stellar.
816	—	— —	*f*	25	11	*n*	1	33	1	*e*F. S. *l*E.
817	—	— —	*f*	26	48	*n*	0	45	1	*c*F. S. *i*F.
818	—	— —	*f*	33	4	*f*	1	7	1	*c*F. S. R. *vglb*M.

III.	1789.	Stars.		M.	S.		D.	M.		Description.
819	Apr. 26	82 Ursæ -	*p*	32	15	*f*	2	19	1	*v*F.
820	—	— —	*p*	29	17	*f*	2	48	1	*v*S stars at less than 1′ *d.* with *v*F. nebulosity between them.
821	—	— —	*p*	12	59	*f*	0	7	1	*c*F. Stellar.
822	—	— —	*p*	6	23	*f*	1	25	1	*c*F. *p*S. *i*R. *lb*M.
823	—	— —	*p*	5	5	*f*	1	18	1	*c*F. *p*L. R. *vlb*M.
	1790									
824	Mar. 8	7(*a*)Hyd.Crat.	*f*	7	26	*f*	1	9	1	*v*F. *v*S. *i*R. *glb*M.
825	10	39 Lyncis	*p*	12	53	*f*	1	31	1	*v*F. S. R. *b*M. *f* of a S*ft.*
826	—	— —	*p*	5	55	*f*	1	56	1	*v*F. S *r.*
827	—	— —	*f*	2	11	*f*	1	29	1	*e*F. *v*S. *ff* a *v*S*ft.*
828	—	Hyd. L. C. 1039	*p*	2	1	*f*	1	11	2	*e*F. *p*S. R. *vgb*M. Stellar. just *p* a *v*S*ft.*
829	17	27 Lyncis	*p*	23	49	*n*	1	30	1	*e*F. *v*S. R. *b*M.
830	—	— —	*p*	10	40	*n*	1	19	1	*c*F. *p*S. *b*M.
831	—	15 (*f*) Ursæ	*p*	12	8	*n*	0	28	1	*v*F. *v*S.
832	—	— —	*f*	9	39	*n*	0	57	1	*v*F. S. *l*E.
833	—	26 Ursæ	*f*	134	8	*f*	1	43	1	*v*F. *v*S.
834	—	74 Ursæ -	*f*	2	4	*f*	1	56	1	*e*F. S. *i*F.
835	—	77 Ursæ	*f*	82	37	*n*	1	52	1	*e*F. S. E. but nearly R.
836	18	17 Ursæ -	*p*	79	17	*f*	0	33	1	*v*F. *v*S. may be a patch of stars.
837	—	— —	*p*	75	32	*f*	0	40	1	*e*F. *v*S.
838	—	— —	*p*	75	10	*f*	0	15	1	*e*F. *v*S.
839	—	— —	*p*	72	22	*f*	3	40	1	*e*F. *v*S.
840	—	— —	*p*	63	56	*f*	1	28	1	*c*F. *c*S.
841	—	— —	*p*	16	9	*f*	1	9	1	*v*F. S.
842	—	43 Ursæ	*p*	5	8	*f*	0	39	1	*v*F. *v*S. R.
843	—	66 Ursæ -	*p*	19	23	*n*	1	52	1	*v*F. Stellar. *np* a S*ft.*
844	—	— —	*p*	16	1	*n*	2	2	1	*v*F. S. *m*E.
845	—	69 (*δ*) Ursæ	*p*	4	55	*n*	1	17	1	*v*F. S. E. par.
846	19	20 Ursæ -	*f*	7	53	*f*	2	28	1	*c*F. S. *m*E. very narrow.
847	—	76 Ursæ	*p*	67	58	*f*	2	50	1	*e*F. *v*S. *i*F.
848	—	69 Ursæ Hev.	*p*	19	5	*n*	2	18	1	*v*F. *v*S.
849	—	— —	*f*	23	58	*f*	0	8	1	*v*F. *v*S.
850	20	76 Ursæ -	*p*	26	56	*n*	3	17	1	*v*F. *p*S.

III.	1790.	Stars.		M.	S.		M.	D.	♀	Description.
851	Mar. 20	76 Ursæ -	*p*	25	25	*n*	0	48	1	*e*F. S. *i*F.
852	—	— —	*p*	16	38	*n*	2	12	1	*v*F. Stellar, *nf* a S triangle of B*ſt.*
853	Apr. 1	30 (φ) Ursæ	*f*	8	55	*n*	1	36	1	*v*F. S. *vglb*M.
854	Oct. 9	72 Pegasi	*f*	15	8	*ſ*	0	23	2	*v*S close *ſt.* with nebulosity between.
855	—	— —	*f*	27	15	*n*	0	3::	1	Two nebulæ, both *e*F. Stellar. dist. 1′ from 30° *ſp* to *nf.*
856										
857	—	σ Fornacis L. C. 285 -	*p*	12	30	*ſ*	1	54	1	*v*F. S. *i*F. *lb*M.
858	10	6 Pegasi -	*p*	24	40	*n*	0	48	1	*e*F. *p*L. *i*R. *vlb*M. requires great attention to be seen.
859	—	— —	*p*	7	56	*n*	0	17	1	*c*F. *v*S. *i*R. *mb*M. near a *vS ſt.*
860	Nov. 2	72 Pegasi	*p*	5	19	*n*	1	7	1	*v*F. S. *lb*M.
861	—	— —	*f*	37	50	*ſ*	0	17	1	*e*F. S.
862	8	1 Lacertæ Hev.	*p*	8	17	*n*	1	19	1	*e*F. *p*L. *i*R. *r.*
863	—	— —	*f*	3	9	*n*	0	48	1	*v*F. *v*S. *mb*M.
864	—	— —	*f*	4	37	*n*	0	50	1	*v*F. S. *m*E. 75° *np ff. b*M.
865	13	96 Aurigæ	*p*	1	9	*n*	1	31	1	*v*F. *v*S. R. *b*M.
866	26	29 (π) Androm.	*p*	27	37	*ſ*	0	20	1	*v*F. *v*S. The *np* corner of a square.
867	Dec. 6	Mayer's Zod. Cat. 20 -	*p*	49	19	*ſ*	1	39	1	*e*F. *p*S. *i*R. *lb*M.
868	—	— —	*p*	39	35	*ſ*	0	42	1	*e*F. *p*S. *i*F.
869	25	44 Piscium	*f*	8	25	*n*	0	55	1	*v*F. *v*S. *b*M. *p.* and in the field with II. 854. *nf.* 2. S*ſt.*
870	—	— —	*f*	12	48	*n*	0	49	1	*v*F. S. *i*R. *vgb*M.
871	28	Mayer's Zod. Cat. 18 -	*p*	8	1	*n*	1	44	1	*v*F. S. R. *vgb*M.
872	—	— —	*p*	5	52	*n*	0	41	1	*v*F. *v*S. *b*M.
873	—	— —	*p*	5	32	*n*	0	39	1	*e*F. *c*L. In the field with the foregoing, and with II. 860.
874	—	57 Aurigæ	*f*	17	56	*n*	1	50	1	*v*F. *v*S. *l*E.
875	—	— —	*f*	21	42	*ſ*	0	7	1	*v*F. *v*S.

III.	1790.	Stars.		M.	S.		D.	M.	Cp	Description.
876	Dec. 29	51 Piscium	*f*	5	44	*n*	1	43	1	*v*F. *p*L. *i*R. *ff* a S/t which is partly involved in the nebulosity.
	1791									
877	Feb. 23	26 Hydræ	*p*	78	56	*n*	0	22	1	*v*F. *i*R. *r.* 2′*d.* almost of equal light throughout.
878	Apr. 2	14 (τ) Ursæ	*f*	9	14	*n*	0	38	2	*v*F. *c*L. R. *mb*M. near 5′*d.*
879	—	73 Ursæ -	*p*	2	39	*f*	1	12	1	*c*F. S. *i*F.
880	—	— — —	*f*	8	13	*f*	1	26	1	*e*F. S.
881		335 Ursæ -	*f*	21	51	*n*	0	13	1	*v*F. S.
882	May 6	9 Ursæ min.	*p*	34	52	*f*	2	0	1	*v*F. *p*L. R. *b*M.
883	—	13(γ) Ursæ min.	*f*	42	41	*f*	1	36	1	*e*F. *v*S. ver. 300.
884	—	— — —	*f*	44	51	*f*	2	22	1	*v*F. *v*S. with 300 *c*L.
885	24	37 (ξ) Bootis	*p*	3	44	*f*	0	35	1	*e*F. *v*S. E. near par.
886 } 887	26	7 Serpentis	*p*	15	32	*n*	0	20	1	Two nebulæ, both *e*F. *v*S. the *p* is the most *n.* dist. 1½′.
888	27	19 (ξ) Coronæ	*p*	6	41	*n*	1	7	1	*e*F. *v*S. R. with 300 *p*L.
889	28	17 (σ) Coronæ	*p*	2	1	*f*	0	52	1	*v*F. S. R. *vglb*M.
890	—	20 (σ′) Coronæ	*f*	8	9	*n*	1	20	1	*v*F. *p*L. *l*E. *lb*M.
891	30	25 Herculis	*p*	3	41	*n*	0	37	1	*e*F. *v*S. R. *lb*M.
892	—	— —	*p*	2	5	*f*	0	9	1	*e*F. S. *b*M.
893	—	44 (η) Herculis	*p*	6	26	*n*	0	8	1	*e*F. *v*S. *i*F. ver. 300.
	1792									
894	Apr. 20	22 (*f*) Bootis	*f*	12	29	*n*	1	15	1	*v*F. *v*S.
895	—	— —	*f*	12	55	*n*	0	47	1	*v*F. *v*S.
896	—	— —	*f*	16	45	*f*	0	25	1	*e*F. S. *vlb*M.
	1793									
897 } 898	Feb. 4	34 (θ) Gemin.	*p*	1	33	*f*	0	31	1	Two nebulæ. The most *n.* and *p.* *e*F. S. The other *e*F. *v*S. dist. 4′.
899	—	— —	*f*	15	18	*n*	1	17	1	*v*F. S. nearly R. *b*M.
900 } 901	—	— —	*f*	36	21	*n*	0	9	1	Two nebulæ just preceding III. 703. Both *e*F.
902	Mar. 8	18 Navis	*f*	10	36	*n*	0	32	1	*v*F. *l*E. *r. b*M.
903	Apr. 6	4 Draconis	*p*	30	48	*n*	0	10	1	*e*F. S. *i*F. *vlb*M.
904	—	— —	*p*	23	25	*n*	0	24	1	*e*F. *v*S. E. mer.

III.	1793.	Stars.		M.	S.		D.	M.		Description.
905	Apr. 7	4 Draconis	*p*	37	3	*n*	0	8	1	*e*F. *v*S. ver. 300.
906	—	6 Draconis	*f*	12	31	*n*	1	8	1	*v*F. E. 2′*l*, $\frac{1}{2}$′*b*.
907	—	— —	*f*	16	26	*n*	1	35	1	*v*F. E. *np ff*. 1$\frac{1}{2}$′*l*, $\frac{1}{2}$′*b*.
908	—	— —	*f*	23	36	*n*	0	10	1	*e*F. *v*S. *i*R. *vlb*M.
909	—	— —	*f*	39	10	*n*	0	35	1	*v*F. *v*S. R.
910	8	37 Ursæ -	*p*	15	47	*n*	0	19	1	*v*F. *p*L. *i*F. *r*. some of the stars visible.
911	—	— —	*p*	11	47	*f*	0	5	1	*v*F. *c*L. *i*F.
912	—	— —	*f*	0	59	*n*	1	27	1	*e*F. *v*S. ver 300.
913	—	39 Ursæ -	*f*	8	14	*n*	1	14	1	*v*F. *v*S.
914	—	— —	*f*	10	29	*f*	0	2	1	*v*F. S. *l*E.
915	—	— —	*f*	25	35	*n*	0	3	1	*v*F. S.
916	9	42 Ursæ -	*p*	48	48	*n*	0	39	1	*e*F. *v*S. Stellar near a S*ft*.
917	—	— —	*p*	15	19	*f*	0	44	1	Two nebulæ.
918	—	— —	*p*	15	10		0	47	1	Both *v*F. *p* S. R. *lb*M.
919	—	— —	*p*	0	1	*n*	2	2	1	*v*F. *v*S. near a *v*S*ft*.
920	—	— —	*f*	19	23	*n*	2	1	1	*e*F. *v*S. E. near mer.
921	—	— —	*f*	24	11	*n*	1	22	1	*e*F. *p*L. E.
922	—	— —	*f*	35	14	*n*	1	11	1	*v*F. *v*S. 2*v*S. stars in it.
923	May 5	Hydr. L. C. 1179	*p*	1	25	*n*	0	5	1	*v*F. *v*S. R. *lb*M.
924	—	6 Hydræ conti	*f*	11	2	*f*	1	27	1	*e*F. S. *r*. ver. 300.
925	12	64 Virginis	*f*	1	18	*n*	1	10	1	*c*F. S.
926	—	— —	*f*	13	5	*n*	1	17	1	*v*F. S. *sp*. a *c*B*ft*.
927	—	19 Bootis Hev.	*p*	0	20	*n*	0	44	1	*v*F. S.
928	13	93 (τ) Virgin.	*p*	26	17	*f*	0	5	1	*v*F. S.
929	—	— —	*p*	9	25	*n*	0	35	1	*v*F. S. E. mer.
930	Sept. 6	53 Aquarii	*p*	27	19	*n*	0	18	1	*e*F. ver. 300.
931	—	— —	*p*	12	23	*f*	0	19	1	*e*F. S. *i*R.
932	—	— —	*p*	8	50	*n*	1	11	1	*e*F. S. *l*E. *f* of a S*ft*. to which it seems almost to be attached, but is free from it. The star is the 1st of 3, making a S triangle.
933	—	— —	*p*	6	7	*n*	0	58	1	*v*F. S. R. *b*M.
934	1794 Apr. 1	Georgian planet	*p*	0	16	*f*	0	2	1	*v*F. This nebula was seen at 9^h 45′, sidereal time, the

III.	1794.	Stars.		M.	S.		D.	M.		Description.
										telescope being out of the meridian.
935	Apr. 19	12 (δ) Hydræ crateris -	*f*	15	11	*n*	0	40	1	*e*F. S. *b*M.
936	Oct. 15	5 (α) Cephei	*f*	7	54	*n*	0	16	1	*v*F. *er*,
	1797									
937	Nov. 22	Neb. I. 274	*f*	25	3	*n*	0	53	1	*v*F. S. *i*R. *b*M.
938	Dec. 10	A double st*	*p*	9	5	*n*	0	10	1	*e*F. *p*L. *i*F.* See I. 273.
939	—	— —	*f*	4	0	*f*	0	35	1	*e*F. S.
940	12	5 Dracon. Hev.	*p*	32	24	*f*	0	49	1	*v*F. S. R. *b*M.
941	—	— —	*p*	8	21	*n*	0	57	1	*v*F. *p*S. 2 S *nf* stars make a a triangle with it.
942	—	— —	*f*	4	16	*n*	0	59	1	*e*F. E. near mer. ver. 300.
943	—	5 (a) Ursæ mi.	*f*	46	2	*f*	0	28	1	Two nebulæ.
944										Both *v*F. *v*S. *r.* dist. $1\frac{1}{2}'$ par.
945	—	35 Draconis	*p*	47	10	*f*	1	17	1	*v*F. S. E. *n* of a S*ft*.
946	20	4 (b) Ursæ mi.	*p*	29	31	*n*	1	57	1	*v*F. *v*S. R.
947	—	— —	*p*	14	39	*n*	0	42	1	*v*F. *c*L. *i*F. *vlb*M. *f* of a *p*B. *ft*.
948	—	— —	*f*	2	20	*n*	1	3	1	*e*F. *v*S. E. near mer.
949	—	— —	*f*	14	44	*n*	2	29	1	*e*F. S. *l*E. near par.
950	—	— —	*f*	24	18	*n*	1	13	1	*v*F. S. *r.* It is preceded by a S. patch of *ft*. which appears almost like this nebula, but more resolved.
951	—	4 Cephei of Bode's Cat.	*p*	21	18	*f*	1	25	1	*e*F. S. better with 320.
	1798									
952	Dec. 9	2 (π') Orionis	*p*	10	20	*f*	1	34	1	Two nebulæ within 1′ of each other; mer. Both *v*F. *v*S.
953										
954	10	8 Ceti -	*f*	17	5	*f*	1	15	1	*e*F. S.
955	—	21 Ceti -	*p*	3	46	*n*	0	4	1	*e*F. *v*S. *i*R.
956	—	18 (ϵ) Eridani	*p*	15	21	*f*	0	58	1	*v*F. *v*S. 2 or 3′ *n* of 2S*ft*.
	1799									
957	June	93 Herculis	*p*	4	8	*n*	1	38	1	Two nebulæ.
958	29			3	59		1	37		Both *v*F. *v*S.

III.	1799.	Stars.		M.	S.		D.	M.		Description.
959	Dec. 19	16 Eridani -	*f*	6	37	*n*	0	26	1	The 2d of 2 *v*F. *v*S. $1\frac{1}{2}'$ *ff*l. 60.
960	—	19 Eridani	*f*	1	19	*n*	1	13	1	*v*F. *v*S. ver. 300.
961	—	— —	*f*	2	43	*n*	0	46	1	*v*F. *v*S.
962	—	— —	*f*	20	51	*n*	1	15	1	*v*F. *v*S. *fp*. 2 *p*B*ſt*.
	1801									
963	Apr. 2	208(N)Camelop. of Bode's Cat.	*p*	157	36	*f*	1	16	1	*e*F. S. *i*F.
964	—	— —	*p*	119	54	*f*	3	51	1	*c*F. S. Stellar. ver. 300. just *p*. *a* S*ſt*.
965	—	— —	*p*	117	22	*f*	2	56	1	*v*F. *v*S.
966	—	— —	*p*	118	0	*n*	0	29	1	*v*F. *v*S.
967 968	—	— —	*p*	72	10	*f*	1	52	1	Two nebulæ. The 1st *v*F. S. The 2d *nf*. the 1st *e*F. *v*S.
969	—	— —	*p*	37	31	*f*	2	39	1	*e*F. S.
970	—	— —	*p*	24	19	*n*	0	28	1	*v*F. *p*L. *r*.
971	—	— —	*p*	20	34	*f*	2	31	1	*e*F. *v*S. R.
972	Nov. 28	50 (α) Ursæ	*p*	5	7	*f*	0	10	1	*v*F. *v*S. R. *b*M.
973	Dec. 6	16 (ζ) Ursæ mi.	*f*	14	15	*n*	1	8	1	*v*F. S. *l*E. mer. *r*.
	1802									
974 975	Jan. 1	22 (ε) Ursæ mi.	*p*	10	49	*n*	0	37	1	Two nebulæ; the preceding *c*F. S. *b*M. the foll. *v*F. *v*S. it follows the 1st a few seconds, and is 3′ more north.
976	May 21	2 (γ) Coronæ	*p*	26	50	*n*	0	2	1	*e*F. S. *i*F.
977	Sep. 26	186 P. Camelop. of Bode's Cat.	*f*	9	49	*f*	1	33	1	*e*F. *v*S. 300 confir.
978	—	— —	*f*	33	19	*f*	0	58	1	*e*F. *p*L. *vlb*M. just *n* of 2 *ſt*.

Fourth Class. Planetary Nebulæ.

Stars with Burs, with milky Chevelures, with short Rays, remarkable Shapes, &c.

IV.	1789.	Stars.		M.	S.		D.	M.	°	Description.
59	Mar. 28	55 Ursæ -	*f*	4	51	*n*	0	28	1	*c*B. S. R. BN. The N is considerably well defined, and the chevelure *v*F.
60	Apr. 12	36 Ursæ -	*f*	8	37	*f*	2	28	2	*v*B. R. Planetary, but very ill defined. The indistinctness on the edges is sufficiently extensive to make this a step between planetary neb. and those which are described *vfmb*M.
61	—	64 (γ) Ursæ	*f*	3	56	*f*	0	19	2	*c*B. BrN with *v*FE branches about 30° *np ff.* 7 or 8′*l*, 4 or 5′*b*.
62	14	— —	*f*	2	27	*n*	1	25	1	*c*B. quite R. A large place in the middle is nearly of an equal brightness. Towards the margin it is less bright.
63	24	69 Ursæ Hev.	*f*	1	24	*f*	1	33	1	*c*B. *c*L. *i*R. *er. vgmb*M. 4′ diam. I suppose, with a higher power, I might have seen the stars.
64	1790 Mar. 4	6 Navis -	*p*	7	41	*f*	1	2	2	A beautiful planetary nebula, of a considerable degree of brightness; not very well defined, about 12 or 15″ diam.
65	5	28 Monocerotis	*p*	51	49	*n*	0	26	1	A pretty considerable star, 9 or 10m. visibly affected with *v*F. nebulosity, of very little extent all

IV.	1790.	Stars.		M. S.		D. M.		20	Description.
									around. A power of 300 shewed the same, but gave a little more extent to the nebulosity. The 22d Monc. otis was quite free from nebulosity.
66	Mar. 18	17 Ursæ -	*p*	16 29	*f*	3 6	1		A small star with a *p* B. fan-shaped nebula. The star is on the *p* side of the diverging chevelure, and seems to be connected with it.
67	—	66 Ursæ -	*p*	0 39	*n*	1 55	1		*p* B. *p* L. R. The greatest part of it equally B, then fading away *p* suddenly; between 2 and 3′ diam.
68		1945 Lyncis -	*p*	4 15	*n*	1 44	1		*v* B. S. exactly R. BNM. and *v* F. chev. *vg*. joining to the N. In a lower situation the chev. might not be visible, and this neb. would then appear like an ill defined planetary one.
69	Nov. 30	26 Aurigæ or 31 Hevelii	*p* *f*	88 24 24 59	*f* *f*	0 11 1 26	1		A most singular phenomenon; A *f* 8m. with a faint luminous atmosphere of a circular form, about 3′ in diam. The star is perfectly in the centre, and the atmosphere is so diluted, faint, and equal throughout, that there can be no surmise of its consisting of stars, nor can there be a doubt of the evident connection between the atmosphere and the star.

IV.	1790.	Stars.		M.	S.		D.	M.	S.	Description.
										Another star, not much less in brightness, and in the same field with the above, was perfectly free from any such appearance.
	1791									
70	Mar. 6	6 Draconis	*f*	50	27	*n*	0	27	2	cB. R. almost equally B throughout, resembling a very ill defined planetary neb. about $\frac{1}{2}'$ diam.
71	May 24	37 (ξ) Bootis	*f*	16	5	*f*	0	44	1	A star 7.6m. enveloped in extensive milky nebulosity. Another star 7m. is perfectly free from such appearance.
	1792									
72	Sep. 15	34 Cygni -	*p*	5	10	*n*	0	28	1	A double star of the 8th magnitude, with a faint south-preceding milky ray joining to it, $8'$, and $1\frac{1}{2}'$ broad.
	1793									
73	Sep. 6	16 (c') Cygni	*f*	2	51	*f*	0	11	1	A bright point, a little extended, like two points close to one another; as bright as a star of the 8.9 magnitude, surrounded by a very bright milky nebulosity suddenly terminated, having the appearance of a planetary nebula with a lucid centre; the border however is not very well defined. It is perfectly round, and I suppose about half a minute in diam. It

IV.	1793.	Stars.		M.	S.		M.	D.	♀	Description.
										is of a middle species, between the planetary nebulæ and nebulous stars, and is a beautiful phenomenon.
74	1794 Oct. 18	7 Cephei -	*p*	24	57	*n*	1	22	1	A star 7m. very much affected with nebulosity, which more than fills the field. It seems to extend to at least a degree all around; smaller stars, such as 9 or 10m. of which there are many, are perfectly free from this appearance. A star 7.8m. is perfectly free from this appearance.
75	—	7 Cephei -	*f*	14	40	*f*	0	46	2	Three stars about 9m. involved in nebulosity. The whole takes up a space of about $1\frac{1}{2}'$ diam. other stars of the same size are free from nebulosity.
76	1798 Sept. 9	3 (ν) Cephei	*p*	10	31	*f*	1	36	1	*c*F. *v*L. *i*F. a sort of BNM. The nebulosity 6 or 7′. The N seems to consist of stars, the nebulosity is of the milky kind. It is a pretty object.
77	Dec. 19	16 Eridani -	*f*	4	56	*n*	0	14	1	A star about 9 or 10m. with a nebulous ray to the south-preceding side. The ray is about $1\frac{1}{2}'$ long. The star may not be connected with it.

IV.	1801.	Stars.		M.	S.		D.	M.	°	Description.
78	Nov. 8	8 Ursæ min. of BODE's Cat.	*p*	25	0	*n*	0	12	1	*c*B. R. about 1½′ diam. Somewhat approaching to a planetary nebula, with a strong hazy border.

Fifth Class. Very large Nebulæ.

V.	1789.	Stars.		M.	S.		M.	D.	°	Description.
45	Apr. 12	64 (γ) Ursæ -	*f*	0	9	*f*	1	23	2	*c*B. *i*F. E. mer. LBN. with F. branches 7 or 8′*l*, 5 or 6′*b*.
46	17	48 (β) Ursæ	*f*	10	4	*f*	0	41	2	*v*B. *m*E. *r*. 10′*l*, 2′*b*. There is an unconnected pretty bright star in the middle.
47	1790 April 1	30 (φ) Ursæ	*f*	10	9	*n*	1	39	1	*v*B. *m*E. *np ff. vgmb*M. 8′*l*, 2′*b*.
48	Oct. 9	Fornacis L. C. 182 - -	*f*	8	7	*f*	0	21	1	*v*B. E. 75° *np ff*. 8′ long. A very bright nucleus, confined to a small part, or about 1′ diam.
49	Dec. 28	41 Persei Hev.	*f*	22	0	*n*	0	15	1	6 or 7 small stars, with faint nebulosity between them, of considerable extent, and of an irregular figure.
50	1793 Mar. 4	Pixidis Na. L. C. 831 -	*f*	35	26	*f*	0	43	1	*v*F. *v*S. *l*E. 15° *fp nf. lb*M. 8′*l*, 5 or 6′*b*.
51	April 6	4 Draconis -	*p*	14	48	*n*	0	20	2	*v*F. *m*E. 70° *np ff*. About 25′*l*, and losing itself imperceptibly, about 6 or 7′ broad.

V.	1801.	Stars.		M.	S.		D.	M.	90	Description.
52	Nov. 28	50 (α) Ursæ	*p*	17	49	*n*	1	30	1	*c*B. E. mer. *vgb*M. About 5′*l.* and 3′ broad; the nebulosity seems to be of the milky kind; it loses itself imperceptibly all around. The whole breadth of the sweep seems to be affected with very faint nebulosity.

Sixth Class. Very compressed and rich Clusters of Stars.

Additional Abbreviations. cl. *Cluster*, com. *compressed*, sc. *scattered*, co. *coarsely*.

VI.	1790.	Stars		M.	S.		D.	M.	90	Description.
36	Mar. 4	6 Navis -	*p*	8	45	*ſ*	1	55	2	A *v.* com. cl. of S, and some L*ſt.* E near mer. The most compressed part is about 8′*l*, and 2′*b.* with many scattered to a considerable distance.
37	1791 Feb. 23	26 Hydræ -	*p*	79	30	*n*	1	0	1	A v. com. and very rich cl. of stars. The stars are of 2 sizes, some considerably L. and the rest next to invisible. The com. part 5 or 6′ in diam.
38	Aug. 25	50 (γ) Aquilæ	*p*	14	50	*ſ*	1	18	1	*c*B. S. *i*F. *er.* Some of the *ſt.* are visible.
39	1798 Mar. 8	ζ Pixidis Naut. L. C. 777 -	*p*	20	89	*ſ*	0	19	2	A cl. of L*ſt.* considerably rich *i*R. above 15′ diam.

VI.	1793.	Stars.		M.	S.		D.	M.	♀	Description.
40	May 12	53 (ν) Serpentis	*p*	48	17	*n*	0	21	1	A very beautiful *e* com. cl. of *ſt.* extremely rich, 5 or 6′ in diam. gradually more compressed towards the centre.
	1797									
41	Dec. 12	35 Draconis -	*p*	22	6	*ſ*	1	71	1	R. *r.* about 3′ diam. *vgb*M. I suppose it to be a cluster of stars extremely compressed. 300 confirms the supposition, and shews a few of the stars; it must be immensely rich.
	1798									
42	Sep. 9	3 (η) Cephei	*p*	13	26	*ſ*	1	61	1	A beautiful compressed cl. of S*ſt.* extr. rich, of an *i*F. The preceding part of it is round, and branching out on the following side, both towards the *n.* and towards the *ſ.* 8 or 9′ in diam.

Seventh Class. Pretty much compressed Clusters of large or small Stars.

VII.	1788.	Stars.		M.	S.		D.	M.	♀	Description.
56	Dec. 16	11 (β) Cassiop	*p*	9	57	*n*	2	61	1	A *p.* com. cl. of S*ſt.* of several sizes, cons. rich. E. near par. 5 or 6′*l.*
57	31	40 Aurigæ -	*f*	8	28	*n*	1	25	1	A compressed cl. of 15 S stars *i*F. 6′ diam. consid. rich.
	1790									
58	Mar. 4	6 Navis -	*f*	5	18	*ſ*	0	29	1	A *p.* com. and rich cl. of S stars *i*R. 7 or 8′ diam.
59	Sept. 11	18 (δ) Cygni	*f*	18	38	*ſ*	1	41	1	A *v.* rich cl. of L*ſt.* considerably compressed, above

VII.	1790.	Stars.		M.	S.		D.	M.		Description.
										15′ diam. by the size of the *ſt.* it is situated in the milky-way, towards us.
60	Dec. 28	47 (λ) Persei	*f*	3	30	*ſ*	0	50	1	A L. cl. of *cL ſt. p.* com. and very rich. *i* R. 7′ diam.
61	—	41 Persei Hev.	*p*	3	8	*n*	0	56	1	A beautiful cl. of L *ſt. v* rich, and considerably com. about 15′ diam.
62	1791 Aug. 21	19 Aquilæ -	*p*	0	26	*ſ*	1	24	1	A S. *p.* com. cl. of stars not very rich.
63	1793 Mar. 3	ζ Pixidis Naut. L. C. 777 -	*p*	2	25	*ſ*	0	24	2	A L. cl. of scattered S *ſt. i* F. considerably rich.
64	4	— —	*p*	20	55	*ſ*	1	9	1	A L. cl. of *ſt.* of a middling size. *i* E. considerably rich. The stars are chiefly in rows.
65	8	2 Navis -	*p*	16	10	*n*	0	38	1	A S. cl. of *vS ſt.* considerably rich and compressed.
66	1794 Oct. 18	7 Cephei -	*f*	16	45	*ſ*	1	7	2	A cl. of cons. com. *vS* and L. stars about 12′ diam. considerably rich.
67	1799 Jan. 30	15 (π′) Canis	*f*	42	33	*ſ*	0	14	2	A cl. of com. stars, considerably rich.

Eighth Class. Coarsely scattered Clusters of Stars.

VIII	1788.	Stars.		M.	S.		D.	M.	♀	Description.
79	Dec. 16	11 (β) Cassiop	f	20	35	n	1	51	1	A coarsely sc. cl. of L*ft.* mixed with smaller ones, not very rich.
80	18	1 Camelopar.	p	41	36	f	1	29	1	A cl. of S. stars, containing one large one, 10; 9m. 2 or 3′ diam. not rich.
	1789									
81	July 18	5 Vulpeculæ	p	2	46	n	2	41	1	A sc. cl. of *c*L *ft. i*F. pretty rich, above 15′ in extent.
	1790									
82	Sept. 11	57 Cygni -	f	1	0	n	0	52	1	A L. cl. of *p*S. stars of several sizes.
83	30	51 Cygni -	p	25	24	f	0	11	1	A cl. of sc. stars, above 15′ diam. pretty rich, joining to the milky-way, or a projecting part of it.
84	Dec. 28	33 (α) Persei	f	9	14	n	1	36	1	A cl. of S*ft.* not very rich.
85	—	41 Persei Hev.	f	2	42	f	0	21	1	A coarsely sc. cl. of L*ft.* pretty rich.
	1792									
86	Sept. 15	34 Cygni -	p	9	48	n	0	15	1	A coarsely sc. cl. of L stars, of a right-angled triangular shape.
	1793									
87	Mar. 8	2 Navis -	p	7	10	f	0	15	1	A small cl. of S. stars, not very rich.
	1799									
88	Dec. 28	46 (ξ) Persei	p	27	13	n	1	29	2	A cl. of coarsely sc. L*ft.* about 15′ diam.

Fig. 1.

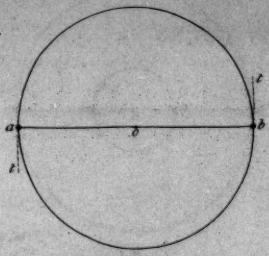


Fig. 2.

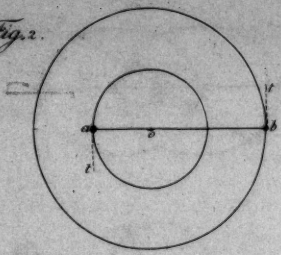


Fig. 3.

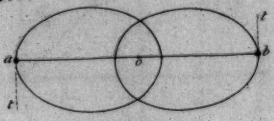


Fig. 4.

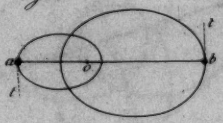


Fig. 5.

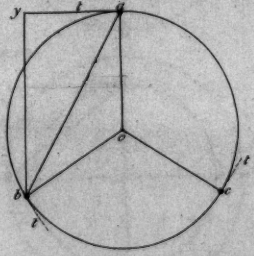


Fig. 6.

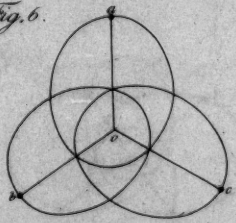


Fig. 7.

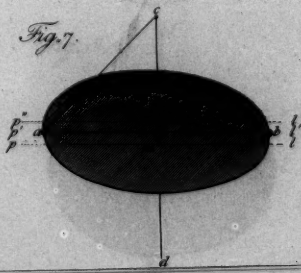
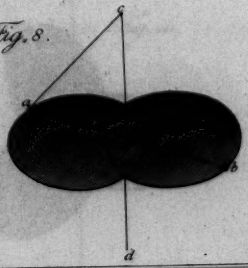


Fig. 8.



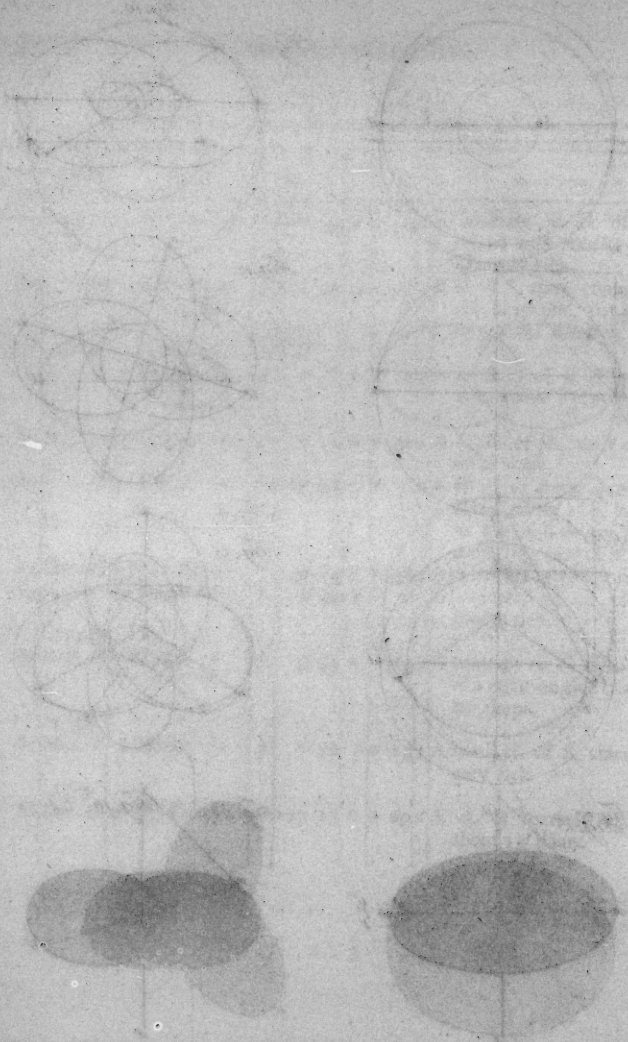


Fig. 9.

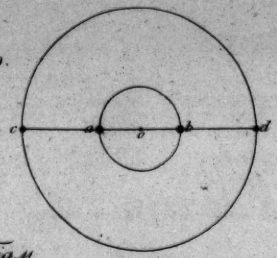


Fig. 10.

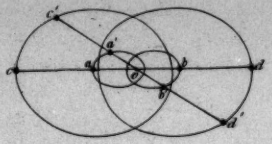
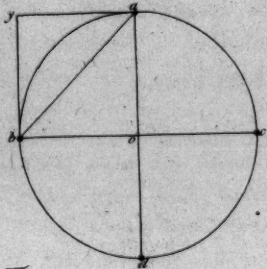


Fig. 11.



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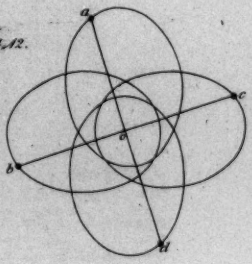

Fig. 13.

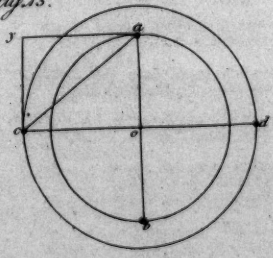

Fig. 14.

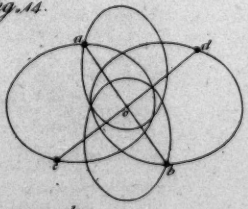

Fig. 15.

Fig. 16.

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	Nova Acta Academiæ Scientiarum Imperialis Petropolitanæ. Tom. XII. Petropoli, 1801. 4°	The Imperial Academy of Sciences of Petersburg.
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	A Series of Engravings to illustrate the Morbid Anatomy of the Human Body. Fascic. VII. London, 1801. 4°	Matthew Baillie, M. D. F. R. S.
	Histoire Celeste Française, publiée par J. Delalande. Tome I. Paris, An 9. (1801.) 4°	M. Delalande, F. R. S.
	Annuaire de la Republique Française pour l'Année X. Paris, An 9. 12°	
	Bibliotheque Britannique. No. 113—120.	Professor Pictet, F. R. S.
	Supplement to a Memoir concerning the fascinating Faculty which has been ascribed to the Rattle-Snake. (Philadelphia, 1800.) 8°	Professor Barton, of Philadelphia.
	Moyens propres à sauver les Equipages d'une Partie des Vaisseaux qui viennent echouer et perir à la Côte, par Ducarne Blangy. Paris, An 9. (1801.) 8°	M. Ducarne Blangy.
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	A Journal of Natural Philosophy, by W. Nicholson. No. 53—57.	Mr William Nicholson.
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	Description et Connoissance générale des Constellations, par J. E. Bode. Berlin, 1801. fol.	
	Astronomisches Jahrbuch für das Jahr 1804, von J. E. Bode. Berlin, 1801. 8°	
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	Introduction à la Science de l'Economie politique, et de la Statistique générale, par G. Leblanc, Paris, 1801. 8°	M. Leblanc.
28.	The Prospectus, Charter, Ordinances, and Bye-laws, of the Royal Institution of Great Britain. London, 1800. 4°	The Managers of the Royal Institution.
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	J. B Van Mons Censura Commentarii a Wieglebo nuper editi, cui Titulus: De Vaporis aquei in Aërem Conversione. Bruxelles, An 9. 4°	———
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25.	Kongl Vetenskaps Academiens Nya Handlingar, Tom. XXI. för År 1800, 3d and 4th Quarter; Tom. XXII. för År 1801, 1st, 2d, and 3d Quarter. Stockholm. 8°	The Royal Academy of Sciences of Stockholm.
	Charts of Meteorological Observations, made at Soho, in the County of Stafford, in 1799, 1800, and 1801.	Matthew Boulton, Jun. Esq.
	Théatre de M. Cailhava. Paris, 1781. Tomes II. 8°	M. Cailhava, de l'Institut de France.
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	Hydrogéologie, par J. B. Lamarck. Paris, An 10. 8°	M. Lamarck, de l'Institut de France.
	J. G. Schneider Historiæ Amphibiorum naturalis et literariæ Fasciculus 2. Jenæ, 1801. 8°	Professor Schneider, of Frankfort on the Oder.
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	Disquisitiones arithmeticæ, Auctore C. F. Gauss. Lipsiæ, 1801. 8°	Charles Frederick Gauss, Ph. D.
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	Literary Antiquities of Greece, by P. Allwood. London, 1799.	4°	The Rev. Philip Allwood, M. A.
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	Bibliotheque Britannique. No. 145—150.		Professor Pictet, F. R. S.
	Captain Cook's Second Voyage, translated into Russian by L. Kutuzov. Petersburg, 1796—1799. 4 Vols.	4°	M. Kutuzov.
	De' Benificamenti delle Terre Pontine, da N. Nicolaj. Roma, 1800.	fol.	Abate Nicolò Nicolaj.
	Mélanges mathematiques, par le Commandeur de Nieuport. 1 Recueil. Bruxelles, 1794. 2 Recueil. 1799.	4°	M. De Nieuport, de l'Institut de France.
May 6.	Mémoires de l'Institut National des Sciences et Arts. Tome 3me des trois Classes. Paris, An 9. Tomes III.	4°	The National Institute of France.
	Hints designed to promote Beneficence, Temperance, and medical Science, by J. C. Lettsom. London, 1801. 3 Vols.	8°	John Coakley Lettsom, M. D. F. R. S.
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	Philosophical Papers, by B. Count of Rumford. Vol. I. London, 1802.	8°	Benjamin Count of Rumford, F. R. S.
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	Calendarium Rotulorum Patentium in Turri Londinensis, 1802. fol.	———
	Taxatio ecclesiastica Angliæ et Walliæ, auctoritate P. Nicolai, IV. 1802. fol.	———
	A Sermon preached in the Chapel of the Magdalen Hospital, by C. P. Layard. London, 1802. 4°	The Rev. Charles Peter Layard, D. D. Dean of Bristol, F. R. S.
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